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SCIENCE AND
HUMAN EXPERIENCE

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by

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CONTENTS

PREFACE

This book, with the exception of the last chapter, is based on a course of four lectures on "The Nature and Scope of Physical Science," given at the Royal Institution in the early part of this year. Portions of the earlier chapters have appeared in *Nature* (March 28 and April 4, 1931) and the bulk of Chapter X in the Imperial College Magazine (*Monochrome*, April 1931). My thanks are due to the Editors of those journals for permission to use this material in the structure of the book.

I have to a large extent retained the "lecture" style on the printed page. The capital "I" therefore appears much more frequently than in ordinary circumstances I should wish, but in present circumstances, which are far from ordinary, this has advantages. The recent rate and direction of development of physics are so abnormal that an impersonal presentation could hardly avoid the character of unintelligibility. The expositor's *interpretation* of the development must perforce be freely interwoven with the texture of the story, and it is of the first importance, therefore, that opinions which are peculiar to him shall bear some clear mark of distinction from conclusions reached by the scientific world in general. It would be unfair to shirk this duty through excess of modesty.

The book has two purposes, which are by no means clearly separated. In the first place it proposes an interpretation—already referred to—of the present position of physics, which seems to me to make rational what at first sight appears fantastic. This interpretation amounts, in a sense, to a philosophy of Science, but having had no systematic training in philosophy, I hesitate to dignify it by so august a name. Terms may have been used with meanings other than those which philosophers generally ascribe to them now, and with this possibility in mind I have tried to prevent misunderstanding by defining them as clearly as possible, using the same word always in the same sense, and illustrating every general statement by at least one example. The book in this aspect is intended for scientists and not as a direct contribution to philosophy, but perhaps I may be allowed to hope that philosophers will find it useful in forming their estimate of modern Science.

The second purpose of the book is to provide, for the ordinary intelligent reader, an account of the remarkable changes in recent

physical thought which have excited so much general interest. I have not, I hope, sacrificed rigidity of treatment to the desire for popular appeal, for since, as has already been indicated, my interpretation of modern physics seems to me to show it as a rational, and indeed almost inevitable, development, the two aims form a natural union. There are two things I would like to say on this point. First, my account of the relativity and quantum theories is exceedingly general. The difficulties of the subject are mainly in the details and have therefore been avoided, but I do not think that, from the point of view of the general reader, any regrettable sacrifice has thereby been made. The general reader is not concerned with the technical devices of the specialist, and one element of the cloud of mystification in which he is often enveloped arises from the attempt to explain to him what would be of little use even if it could be grasped. Secondly, I have treated the subject historically, because I think it is only by seeing how the new ideas have evolved from older ones that their naturalness can be recognised. The other element of the cloud just mentioned is created by the presentation of the new ideas as a bolt from the blue, destroying at a blow the familiar beliefs of the past. That is not how the new ideas entered physics. They emerged gradually from the thoughts and labours of former generations, and now that they have come to light we can recognise in them the natural blossoming of seeds of thought planted long ago. It is in the same way, I think, that they should be encouraged to emerge in the minds of non-scientific thinkers.

I have had occasion frequently in the book to discuss the writings of two men whose scientific achievements and consummate skill in exposition have been largely responsible for the present widespread interest in physical Science: I refer, of course, to Sir Arthur Eddington and Sir James Jeans. As my comments have generally been in the nature of adverse criticism, I take this opportunity, not of expressing, but of stating how difficult it is to express my admiration of their work and my personal indebtedness to it. Without it this book would probably never have been written; certainly it would not have had its present form. For in reading what these men have written I have found my acceptance of their positions opposed by an instinctive rebellion which I am convinced is at bottom rational, and this book is largely an attempt to formulate the sources of that rebellion. The path of opposition to those whom one so greatly admires, and for the greater part of whose view of modern physics

one is in respectful agreement, is not a pleasant one to take, and I hope this general comment will effectually prevent any misunderstanding which might otherwise have arisen.

It is impossible to acknowledge in detail the full extent of my indebtedness to other writers; I do not know it myself. In connection with the historical portion of the book, however, I wish particularly to mention Professor Burtt's excellent volume, *The Metaphysical Foundations of Modern Science* (Kegan Paul, Ltd.), which, although I have not followed it slavishly, has been of the greatest help. Some of the quotations from Copernicus and Galileo, which I have not found elsewhere translated into English, have been taken therefrom.

A word should be added about the last three chapters of the book. They are—I hope obviously—not intended as a systematic treatment of Art, Criticism and Religion, but merely illustrative of the kind of way in which I think Science can be of use or of harm in non-scientific domains. The chief problems of modern thought seem to me to resolve themselves into three: first, the establishment of a clear distinction between the scientific and non-scientific fields of mental activity, and an understanding of the relation between them; second, the thorough purification of each field from material and methods of operation properly belonging only to the other; and third, the free use in each field of those instruments and products of research which, although belonging to the other, have legitimately a general application. It is only by dealing with these fundamental problems that the particular difficulties which beset our time can be finally overcome.

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INTRODUCTION

It is a trite saying that we live in an age of Science, and, like all platitudes, it is usually accepted with little apprehension of what it means. What is this Science which not only shapes the material environment of our lives but, much more profoundly, permeates and forms the predominant constituent of the intellectual atmosphere which we breathe? This is a question which needs almost as much care to state as to answer it. We are not to think of a framework of doctrine within which scientific activity must always operate. There is no faith once delivered to or by the scientific saints, and those who attempt to set final limits to what Science can legitimately do or say are merely making an arbitrary classification of intellectual inquiry. From the point of view which we shall adopt in this book we see no such limits; we simply contemplate the achievements and tendencies of Science and try to express its essential characteristics. We try, in fact, to see what Science *is*, with no thought of that mythical thing which it *ought* to be.

The question clearly is one of paramount interest, yet, paradoxically enough, the man whom perhaps it least concerns is the scientist. The working scientist, for the most part, carries on his particular research unaffected by any thought of whether he is obeying or enlarging the scientific tradition. This is sometimes brought against him as a reproach; he is a mere empiricist, working mechanically in his little microcosm of a laboratory, unaware of the relation of his labours to the larger universe of philosophic thought. The charge is, I think, unjust. The absorption of the scientist in his specialised problems is due, not to ignorance or shortsightedness, but to inarticulateness. Every experimentalist, at least, worthy of the name knows by instinct which is the right road to take, and he is usually too busily engaged in taking it to reflect on the sources of his knowledge or on the surrounding country. And it is well that he is, for this "blind understanding" is not only a safeguard against error but allows of natural developments which a more enlightened comprehension might fear to undertake. However liberal-minded and free from convention we might be, there is inevitably something of a restricting character in a formal statement. If we can say what Science is, we tend to keep it so.

But while the scientist can afford to neglect our question, it is otherwise with the philosopher and the ordinary general thinker. The philosopher clearly must ask it, for Science is the most active and perhaps the most disconcerting of his intellectual children. Historically, Science is a special kind of philosophy, and indeed the inquiry into the nature of Science, if it is to be at all thorough, must be a metaphysical inquiry; perhaps it is not too much to say that it is the most profound metaphysical problem of our day, as some philosophers have recognised. Nevertheless, it is not for philosophers that this book has been written. Unfortunately, the nature of philosophy is such that one cannot treat satisfactorily of a single subject without having, as it were, a whole system up one's sleeve, and that is an equipment with which I regret that I am not provided. What I shall try to face is the much less ambitious task of putting forward, for the consideration of the ordinary intelligent person, a view of the field of Science surveyed from the most advantageous view-point I have been able to find.

By the "field" of Science I do not mean merely the area on which Science is at present actively engaged, but the whole region which, with her present implements and methods of working, she seems fitted to exploit. For this is, after all, the most vital concern of the unspecialised thinker, who is most conscious of the activities of Science through the inroads which it makes into his æsthetic, moral and religious ideas. He wants to know, perhaps with mingled hope and fear, how much farther Science can go in prescribing the character of the world in which his lot is cast. He is interested to know that the universe is a bigger thing than his forefathers imagined, but he is still more interested in the question whether it is a better thing. He is grateful for scientific sanction to contemplate a wider horizon than his ancestors saw, but he is apprehensive lest the same authority should next declare it to be a mirage. He wants to know, therefore, what this Science is in itself, what power for both construction and destruction is given to it, so that he may employ it in building his own intellectual habitation on a foundation which it is powerless to undermine. It is towards the solution of this problem that in this book I shall attempt to make a preliminary contribution.

I say "preliminary" and not "final" contribution, not chiefly from modesty or any regard to the more thorough metaphysical inquiry which is so important, but mainly because I do not think that a

final statement of the limitations of Science is possible at the present time. If anyone thinks otherwise, I would refer him to the revolution in fundamental physical ideas which this century has seen, and beg him to consider, first, whether thirty years ago he would not have been equally ready to dogmatise, and secondly, how, if so, his conclusions then would appear at the present time. I think that whenever one meets with the statement that Science can never affect this, or that, of anything which is a part of our common experience, he may safely disregard it. The field of Science, as I have defined it, is perceptibly bounded only by a horizon. It may be that the field is flat, in which case the horizon is a real limit; but, on the other hand, if the common metaphor of the "sphere of activity" of Science can properly be introduced, who can say how far the field extends? The contribution of this book is preliminary in the sense that it accepts the visible horizon as a boundary. It is not useless on that account. The region at present covered by Science is but a small part of the whole area, and if we may not say that anything is eternally immune from the scientific plough, we may, on the other hand, definitely assert that ground hitherto undisturbed may yet feel its teeth, and perhaps we may hazard a guess as to what the crop will be. It is with this end in view that the following pages have been written.

The course which I propose to take is briefly as follows. I shall suggest a definition of the scope of Science which seems to me to include the essential nature of scientific activities at the present time. After commenting on some terms in this definition, I shall trace in outline the gradual progress of physical Science from the Middle Ages, with special reference to the remarkable developments which our generation is witnessing. It is only from regrettable necessity that I confine myself to physical Science. The definition which I give I believe to be applicable to all departments of Science, the unity of which is, I think, far more important than their differences. But since one mediocre brain and half a lifetime's study are pitifully inadequate to substantiate the definition for the whole subject, I have confined myself to the department of which I am least ignorant. It is all the more important, therefore, to emphasise at the beginning my strong conviction that the boundaries which separate the physical, biological and psychological sciences are only temporary conveniences which will one day disappear, perhaps as completely as the boundary between chemistry and physics has

already done. Any statement of the scope of Science which is based on only one department is therefore necessarily inadequate, and everything which I shall say of physical Science is consequently to be regarded as *illustrative* of the general definition and not as a comprehensive demonstration of it. Finally, I shall try to estimate the possible influence of Science in realms of thought which are generally considered as extra-scientific, by a discussion of the nature of poetry, criticism and religion.

What, then, do we mean by Science? I take it to be *the recording, augmentation, and rational correlation of those elements of our experience which are actually or potentially common to all normal people*. The phrase cannot boast of elegance, but at the moment rigidity and conciseness are the important considerations: let us examine it a little.

In the first place, by experience I mean everything of which we are conscious except rational conceptions. I think of consciousness as composed of reason on one hand, and all sensations, feelings, emotions, passions, and the like, on the other; the latter constituting what I mean by experience. Whether this distinction can be maintained in a strictly philosophical sense I do not know, but, as I have said, I make no attempt to be a philosopher and the practical distinction is clear enough. I exclude no experiences from the dominion of Science except those which are not actually or potentially common to all normal people. In judging of what is common, however, we must have regard to particular events, and not to life as a whole. For instance, there may be no one who has no sense of beauty, so that there is a sense in which the experience of beauty is common to all normal people. But it is certainly not true that any particular object is invariably considered as beautiful or otherwise by everyone, and we must therefore exclude the experience of beauty from scientific consideration. For convenience, I shall henceforth speak of experiences susceptible to scientific treatment as "common experiences," and the remainder of experience (including, for example, the sense of beauty) as "individual experiences." Perhaps a more familiar way of describing the experiences susceptible to scientific treatment is to say that they are the observations made by means of our five senses. I do not describe them in that way, however, because that would accentuate an irrelevant aspect of the matter. It is not the way in which we get our knowledge

that fits it for scientific treatment, but the fact that it is universal, that we can discuss it with a mutual understanding. The more general definition also leaves room for the inclusion of knowledge obtained through other channels if that can become common to all.

Again, it is necessary to take account of potential as well as actual experiences, because Science often has to include among its data a much wider field of experience than that which in fact exists. It is not true that everyone at every moment is subjected to all possible experiences, yet Science virtually assumes that this is the case. For instance, Kepler's first law states that the orbit of a planet is an ellipse with the Sun at one of the foci. There are moments when Venus is observed by no one, and for all that actual experience tells us, the planet might at such moments be anywhere. Nevertheless, we assume that if appropriate means of observation were employed, Venus would be found at such times at the places indicated by the law of gravitation, and we have no hesitation in accepting Kepler's law, with inconsiderable modifications, as true Science.

This is probably the simplest example of the assumption, but there are other examples which are much more subtle. Until recently it was assumed that the relative velocity of two bodies was simply the difference between their velocities with respect to a third body. We now believe that this is very far from the truth when the velocities are very high. We have never tried the experiment, but unless it is so the whole concept of velocity is removed from scientific consideration, because we cannot reach a common agreement about it. The only way of reconciling the observations of the motion of the Earth made by observers using different methods is to assume that the law of composition of velocities is different from what was previously thought. It is then found that the reconciliation can be made without violating any of our actual experiences, although it requires several modifications of what we previously thought were potential experiences. These modified potential experiences then become part of the body of Science, although they may never become actual. It should perhaps be said that by "potentially" common experiences I mean experiences which may or may not differ in degree from those which are actually common, but can never differ in kind; and, further, that their inclusion is not a loophole for the admission of experiences which may become

common through possible future developments of the human race, but sanctions only those experiences which are potentially common now. In brief, "potential" experiences are those which are not actual only through accidental circumstances and not from inherent character.

The last example brings us to the rational correlation of experiences. By this I mean what is often spoken of as "explanation." But to say that we have "explained" a phenomenon is to suggest a kind of finality which is unattainable by Science. Every scientific "explanation" will be found on examination to be the establishment of a relation between the phenomenon "explained" and other phenomena. Thus, when Franklin "explained" lightning he simply showed that it was connected with electrical experiments in the laboratory. A relation was thus established between those experiments and the lightning flash, so that knowledge of one phenomenon was available for interpreting the other: the phenomena were *co-related* with one another. This correlation contained all the "explanation" that was given. Whatever was mysterious about the electric spark remained mysterious about the lightning flash.

Correlation is perhaps the most important function of Science. It not only makes our multifarious knowledge mentally manageable but enlarges the field of actual and potential experience and, what is still more important, reveals a unity in experiences which are superficially quite distinct. Scientists will sacrifice a great many preconceptions in order to correlate observations rationally; they fear the fantastic much less than the unconnected. Witness, for example, the apparent extravagances of relativity. Among the highly theoretical physicists, in fact, there is often a temptation (I won't say they yield to it) to turn aside from an obstreperous observation in order to preserve a rational correlation from destruction. Indeed, it sometimes seems that there is more sorrow among the mathematicians over one hard fact than over ninety and nine just theories that admit of no testing. But however that may be, Science is preeminently engaged in correlating observations with one another, and so it comes about, among other things, that to the modern physicist, the whole of material Nature is a manifestation of the transformations of one substance—energy. The significance of the process of correlation we shall consider at length.

It may be observed that our definition is quite consistent with

Huxley's description of Science as "organised common-sense," though it attempts to express the idea of that phrase more rigidly. At the same time, we must remember that common-sense when organised is often not recognisable as common-sense, for it is usually the most unorganised thing in the world. This paradox has led in recent times to the rejection of Huxley's dictum by some writers, but I do not think they have allowed sufficiently for the transforming power of organisation.

The elucidation of our definition is not complete without a reference to the recording and augmentation of experiences. Recording is not merely a placing on record, but an *orderly* placing on record. In the more complex sciences, such as biology, this involves the very important and difficult process of classification. By "augmentation" of experiences we are to understand what is usually expressed by the word "discovery." This word, however, has become invested with a certain glamour. It carries with it a suggestion of something momentous, so that we hesitate to employ it in speaking of the mass of severely detailed data with which Science deals. All data, of course, are not equally important in practice, but in principle it is impossible to make a distinction; there is no last or first. The trifle of to-day becomes the centre of attention to-morrow. It is an essential part of the operations of Science to make actual the whole field of potential common experience, and although in practice the contents of that field are carefully chosen, that is a matter of expediency and not of principle.

One more word of explanation. I have restricted the subject-matter of Science to the experiences of *normal* people. It is difficult to define normality in this connection, but I think there will be no confusion in practice. The blind are obviously excluded, for Science deals largely with experiences which do not come to them. The colour-blind and deaf, however, curiously enough, may be admitted tentatively, for they can employ instruments which make up for their defects. In theory, perhaps, this is true of the blind also if they possess the other senses, but the practical difficulties of admitting them to full fellowship are far from being overcome. The lunatic, the lover, and the poet, while they are acting in their official capacities as Shakespeare describes them, are certainly excluded—not through defect but through excess of experience. Mystics and visionaries are in like case. Despite the impossibility of generalising, there will probably be no difficulty in any

actual case of assessing the value of any particular person's claim to normality. ✓

It will be noticed that it is not a question of whether experiences are *real or illusory*: it is simply a question of whether they are common or not. Normality is definable as conformity to the ordinary type of humanity, and there is no need to discuss whether the experiences of the ordinary person are more or less real than those of, say, the mystic, for their universality is all that is relevant. We need not make Lincoln's assumption that you cannot fool all the people all the time. We simply say that if you can, then Science may deal with foolishness but otherwise it may not.

This question of normality makes it necessary for us to be a little more precise than we have been about the distinction between common experiences and "individual" experiences such as the sense of beauty in a particular object. This distinction, though sharp enough to be apprehended without difficulty, is nevertheless one of degree and not one of kind. The abnormal people to whom I have just referred remind us that common experiences are not universally common, and, on the other hand, individual experiences are not always strictly individual. Even Art critics sometimes agree, and their experiences of beauty in objects are in such cases common experiences if the unappreciative section of humanity is branded as abnormal. This point will come up for more special consideration in Chapter IX, and for that reason, as well as on account of some recent attempts* to make a more absolute division between common and individual experiences, it is desirable to make our ideas on the matter as clear as possible.

Speaking with absolute strictness, we cannot say that any experiences are common; no man can enter into another's mind and feel what is going on there. What we call a common experience is simply one of which the descriptions by different people agree in every particular. When that is so, the experiences are fitted for scientific treatment. Thus, for example, we may call the experience of seeing a chair a common experience, but no one can compare his perception of the chair with that of another; for all he knows, the chair may appear to the other as a gate appears to him. But what he does know is that everything he says about the chair is assented to by the other. That establishes a correspondence between the experiences of the two persons, but it does not establish identity.

* e.g. L. Hogben, *The Nature of Living Matter*.

Now the individual experiences do not satisfy this condition to anything approaching the degree achieved by the common experiences. There may be a large measure of agreement that *Lycidas* is a finer poem than *Casabianca*, but a large number of people could be found who would prefer *Casabianca*; and if the nature of the preferences of either group were inquired into, considerable disagreement would be found about the qualities of the poems to which value was attached. When, therefore, we try to establish a correspondence between the æsthetic experiences associated with a given work of art, we find that the number of persons taking part in it gets fewer and fewer as finer and finer details are taken into consideration, until ultimately we are left with one or two "normal" people and a world of "abnormal" ones. It becomes fantastic to speak of such experiences as "common" experiences, and the practical distinction between them and the experiences dealt with in Science, with respect to which relatively very few people are abnormal, is thus very clear. Nevertheless, it is essentially a *practical* distinction; we must not assume that it is a fundamental one. The situation may be summed up as follows. We classify people as normal or abnormal differently according to the experiences we are considering. There is one set of experiences with regard to which the number of abnormal people is negligible; with regard to all other experiences, the abnormal people exceed the normal ones in number. We choose the former set of experiences as the field of Science.

There is another point which should be mentioned before we proceed. The existence of a general agreement in the descriptions of common experiences makes it possible for us to ascribe those experiences to objects existing in an external world. In so doing we make no pronouncement at all on the hoary philosophical problem of subjectivism and objectivism. The postulation in this book of an external world is only a convenient way of acknowledging the fact of general agreement. Clearly, if we all agree about the characteristics of a set of experiences, we can speak of those experiences as arising from something independent of us without becoming involved in contradictions, and that is all that we require in Science. Whether that something is actually external to us or not is a question for philosophers to discuss; Science can proceed without caring about the answer. In speaking, therefore—as we shall occasionally do in what follows—of objective existences, it must be

understood that we presuppose no particular answer to this question, but merely indicate the fact that the experiences referred to are common ones. Individual experiences cannot consistently be referred to external objects; common experiences can consistently be so referred, but need not necessarily arise therefrom.

Returning now to our definition, we may notice one or two implications which it is worth while to consider. In the first place, the definition includes no *a priori* assumptions. Science, in approaching the world of common experience, is prepared to accept whatever it finds: it does not set out with any preconceptions at all. This is not universally acknowledged. One often meets with statements that Science, by its very nature, presupposes this, that, or the other. Let us examine one or two such statements.

"There can be no living science," writes Professor Whitehead,* "unless there is a widespread instinctive conviction in the existence of an *Order of Things*, and, in particular, of an *Order of Nature*." It is true that this conviction has played a leading part in the actual history of Science—most conspicuously, perhaps, in the ecstatic faith of the great Kepler—but the idea that there *can* be no living Science without it is, I think, not only untrue but inconsistent with the essential characteristics of Science. (By "Nature," of course, I mean the sum of the common experiences just discussed.) "Nature," says Galileo, "nothing careth whether her abstruse reasons and methods of operating be, or be not exposed to the Capacity of Men," and it is hardly a sufficient reply to say that the conviction referred to was not necessarily expressed but instinctive. It is surely a fundamental quality of the scientific attitude to Nature that we assume nothing about her ways, but simply inquire. The fact that we have discerned an order in Nature is therefore a genuine discovery so far as our actual experiences are concerned. But there is this much truth in the dictum, that we assume that our *potential* experiences will take part in the same order. We believe that Venus is in her assigned orbit when we are not looking at her, and we assume that the iron in the structure of a building would give us the same spectrum as a piece of iron in our laboratories, without endangering our liberty by tearing it out to see. But that faith is not instinctive. It is based on the universal order which we have found among actual experiences. Venus might have been as capricious as her classical namesake, but we have discovered that she is not. Moreover, such a

* *Science and the Modern World*, p. 5.

faith is of no importance. In Science we can be indifferent about what *would* happen *if* we did such and such a thing, so long as we can depend on what *will* happen *when* we do it.

Another notion which is commonly held is that Science is pledged to monism; Dean Inge will express it for us very effectively. "Of one thing we may be certain," he writes,* "Science will never renounce the attempt to bring everything under a single system of laws. Science must be monistic, for under any other dispensation science could not exist. The dualism of nature and supernature is intolerable to science."

Now the process of rational correlation of experiences is, of course, an effort to construct a monistic interpretation of those experiences, but it by no means follows that under any dispensation other than monism Science could not exist. In so far as we succeed in correlating our experiences we are progressing towards monism, but it may be that a time will come when our progress in one or more directions will be arrested. Science may fail, for example, to correlate the phenomena of animate with those of inanimate matter, but it will not therefore cease to exist. It will continue to extend its correlations within each of the two spheres of activity, in spite of the fact that they may be fundamentally independent. It is true that, as Dean Inge says, Science will never *renounce the attempt* to bring everything under a single system of laws, and the reason is easy to see. When once a phenomenon is brought within such a system it is brought there for ever. Nothing can then alter the fact that it is rationally correlated with other phenomena obeying those laws. The laws may be reformulated, but the whole set of phenomena will then merely pass into the new regime together. But a phenomenon which is outside the system is not necessarily outside for ever. We have no *a priori* knowledge that it is inherently independent, and so Science continues the attempt to absorb the phenomenon into its system of correlation although it knows quite well that it may not succeed. The aim of Science, in short, is to see how far it can establish a rational correlation between common experiences. It may ultimately be able to string those experiences together into a single network, or only into twenty independent networks; it all remains to be seen. But whatever the result may be, it is not assumed in the current activities or achievements of Science.

A third contention, which is fully justified when properly expressed,

* *Science, Religion and Reality*, ed. by Joseph Needham, p. 371.

may be mentioned here because the expression of it is frequently improper. It is sometimes claimed that in the scientific investigation of Nature the assumption is made that such things as beauty and moral values do not exist. This, however, is untrue. Science leaves these things out of account because they are not common experiences but individual ones. It makes no assumption about their existence, whatever "existence" may mean in this connection; it simply ignores them. It is unscientific to say that they exist, and equally unscientific to say that they do not: the scientific attitude towards them is to say nothing. Individual scientists, of course, may express opinions on the question, but their opinions are not Science. The truth embodied in the contention is that Science is not the only possible intellectual attitude towards our experiences taken as a whole. Science selects its field as that of the experiences which are common to all normal people, but it makes no assumption that that field includes the whole of experience, and it explores that field without prejudice of any kind. To look for æsthetic or moral values as possible discoveries of Science, as is sometimes done,* seems to me to reveal a misapprehension of the very nature of Science. We cannot expect to find these things as the goal of scientific inquiry, because they are excluded at the beginning.

Turning now to another aspect of our definition, we may notice that it says nothing about the *methods* used in Science; it is concerned with ends, not with means. As a historical fact, of course, Science has almost invariably restricted itself to the familiar processes of *observation* and *experiment* (which is simply observation of what happens under prescribed conditions) for the accumulation of her data, and to *abstraction* and *hypothesis* for their rational correlation. I think, however, that this has arisen more from expediency than from ultimate necessity. It is conceivable that Science might be advanced by other means—e.g. clairvoyance—but if such means are employed their indications must always be confirmed by common observation before they are accepted as a part of Science. This, of course, is no more than we demand, or should demand, of a hypothesis. The essential point is that Science is an achievement and not a method of achieving.

Let us look a little more closely at the methods of abstraction and hypothesis, for a great deal of what follows depends on the distinction between them. Abstraction is the detection of a common

* e.g. Jeans, *The Mysterious Universe*, p. 149.

quality in the characteristics of a number of diverse observations: it is the method supremely exemplified in the work of Newton and Einstein. Newton, for example, gave us "laws of motion." Now motion is not an experience; what we observe are moving bodies. Motion is an abstraction, a quality conceived to be possessed by all moving bodies, however much they may differ in size, shape, colour, beauty, virtue, or anything else. The laws of motion express the characteristics of this common quality, and they are therefore a rational means of correlating a vast body of common experience.

A hypothesis serves the same purpose, but in a different way. It relates apparently diverse experiences, not by directly detecting a common quality in the experiences themselves, but by inventing a fictitious substance or process or idea, in terms of which the experiences can be expressed. A hypothesis, in brief, correlates observations by adding something to them, while abstraction achieves the same end by subtracting something. For example, in the early part of the last century the planet Uranus was observed to be moving in an unexpected way: its path was not that which had been calculated on the basis of Newton's abstractions. Now to correlate the motion of Uranus with that of the other planets by a new abstraction in the year 1845 would have been a terrifying business. The simpler plan was adopted of supposing the existence of another planet which, with Uranus, obeyed the laws of motion abstracted from the behaviour of the more familiar planets, and the problem was solved.

A hypothesis of this kind serves Science in two ways. First, it brings about a rational correlation of the experiences in question; and secondly, it can be made the object of direct observation and so, if it is thus verified, augment our experiences. The hypothesis, we say, has been *proved to be true*. This is, of course, what occurred in the example just given, which led directly to the discovery of the planet Neptune. But there are other hypotheses which cannot be thus verified. For example, many of the other diverse phenomena of the solar system can be correlated by assuming that a few thousand million years ago a star passed close to, or collided with, our Sun. Unless we can live through past times and watch events from a suitable view-point elsewhere in the universe, it is impossible to see how this hypothesis can be verified by observation. It might indirectly augment our experiences by suggesting certain inevitable or likely consequences of such an event, which we might look for and

find. But that would not bring the hypothesis itself into the realm of experience; it would simply mean that it was capable of correlating a wider field of experience than that which gave it birth. It should be added that this is not the only—nor, at the moment, the most conspicuous—type of unverifiable hypothesis, but we shall return to this point later.

The mental processes involved in abstraction and the formulation of hypotheses can be traced back through the ages. What is it, then, that is specially characteristic of their employment in Science? I think it is the requirement that they must always start and, as far as possible, end in observation. In the succeeding chapters we shall trace the development of these methods from the Middle Ages to the present time, but before leaving our definition it is desirable that we should consider the relation of psychology to Science. Psychology is a study which is sometimes regarded as scientific and sometimes not. The way in which we shall regard it depends, of course, on what we mean by Science, and a brief discussion of the question here will not only smooth the approach to problems to be considered later, but will also serve still further to elucidate the nature of our definition of Science.

From our point of view psychology may be definitely scientific when it deals with behaviour which is open to common observation. The recording and rational correlation of such behaviour, either within its own limits or with other elements of our common experience, is essentially a scientific process. This statement must not be taken to refer in any particular way to the so-called "behaviourist" school of psychology. In the first place, it does not preclude the possibility that other kinds of psychological study may be equally scientific—that possibility we shall consider in a moment—and, in the second place, it does not mean that in the study of behaviour such abstractions as "mind" and "will" need necessarily be avoided as, I believe, the behaviourist school holds. Science, as we have seen, is not concerned with the methods used to achieve its ends provided the ends attained are scientific, and it prohibits the use of no conception, abstract or hypothetical, which may be found useful. In so far as psychology is the study of common experiences and is concerned with their recording, augmentation and rational correlation, it is true Science.

The procedure in psychological study is, in fact, very similar to that adopted in physics. Mind is an abstraction of the same general

character as matter or motion. There are individual minds just as there are individual pieces of matter, and, furthermore, we can speak of qualities (such as will) associated with mind in general just as we can speak of qualities (such as mass) associated with matter in general. The latter qualities are metrical in character, and we study them by the use of mathematics; the former qualities are non-metrical and the laws of mathematics are inapplicable to them. This difference, however, as we shall try to show in Chapter VII, is superficial compared with the fundamental resemblance.

Language is imperfect, and the use of the word "mind" in psychology suggests that the mind is studying itself. In so far as psychology is scientific, however, this is not so. The mind which studies is essentially subjective: the mind which is the object of study is an abstraction from observed behaviour and is no more subjective than matter; like matter, it is simply a concept invented for the purpose of rationalising observations, and it connotes nothing that is not observable.

Psychology, of course, does not end there; it does attempt to study the subjective mind. It can do so, however, only through individual experiences—such as dreams, meditations, and other "mental states"—and to meet with these it must leave the domain of Science. There is nothing, of course, to prevent it from using scientific methods in this extra-scientific work, and in fact it often does so, but we have already seen that Science is not characterised by the methods used. It is possible to place this "individual" psychology, as we may call it, on a scientific basis by substituting for the individual experiences the descriptions of those experiences given by the individual in question, for such descriptions are statements which may have the same meaning for everybody. When they are available the experiences themselves can be dispensed with, and whatever unification or coherence is arrived at is characteristic of the statements only.

In some cases individual experiences are accompanied by bodily symptoms which may act as substitutes for the statements just referred to. By purely psychological treatment, without any direct attention to the body, the symptoms can often be removed and the patient restored to health. The principles involved here are scientific in so far as they are concerned with the symptoms. The mental states which are assumed in the course of the treatment, and are said to be treated, are hypotheses—perfectly legitimate hypotheses,

no doubt, but still merely convenient aids in the change of the bodily condition. No statement about them is scientific unless it can be justified as a means of describing or correlating conditions which are potentially open to common observation.

It is claimed by expositors that psycho-analysis is a Science because it presupposes the operation of determinism throughout its field of study. In so far as it does so, however, it proclaims itself non-scientific, for Science, as we have seen, starts with no presuppositions about its data. The highly individual experiences with which psycho-analysis is mainly concerned are of the nature of those mentioned above, and whatever methods it employs it cannot make its study of them scientific. It is a science only in so far as it studies individuals' accounts of those experiences, and its results have scientific validity only in respect to those accounts.

This distinction between experiences and the statements of them may seem a mere quibble, but in reality it is much more than that. The statements satisfy the only criterion of truth recognised in Science; namely, that every normal person is agreed about them. Before we can say the same of the individual experiences corresponding to the statements we must assume that the narrator is not lying or unwittingly misrepresenting his experiences. We cannot check this assumption because we have no criterion for deciding the matter. It will be clear, I think, that the necessity for such an assumption makes the distinction between individual experiences and the statements of them a radical one, and is quite sufficient to justify the rejection of the one and the acceptance of the other as valid material for scientific treatment.

The superiority of data to method as a means of characterising a department of intellectual activity is not universally acknowledged, and an example might not be out of place. A well-known logician claims that if one false proposition be granted, anything can be proved. There is a story that he was once challenged to prove, having given that $1 + 1 = 3$, that Mr. A was the Pope. The proof was as follows. "Take 1 from each side of the equation. Then $1 = 2$. Mr. A and the Pope are two; therefore they are one." Now shall we call this mathematics or nonsense? So far as method goes it is undoubtedly mathematics, but the datum is nonsense. I think it will be agreed that "nonsense" is the better description; that is to say, the ground of classification is the character of the data and not that of the method.

It ought not to be necessary (though it probably is) to say that in regarding certain branches of psychology as unscientific, we are in no respect passing judgment on their dignity or validity. That is a matter with which we have no concern. It is commonly assumed, however, that to say that a thing is not scientific is to condemn it. Such an assumption arises from an implicit idea that Science and superstition are the only possible intellectual attitudes to the universe. It is the primary purpose of this book to mark off the field of Science from other fields of research, and if this Introduction has not made it clear that when this is done there is still some territory left which is not given over to vain fancies, it has failed in its purpose. If, however, it has been more successful we may proceed with the next part of our task, which is to trace the rise and growth of modern physical science out of the intellectualism of the Middle Ages.

SCIENCE AND HUMAN EXPERIENCE

CHAPTER I

COPERNICUS AND GALILEO—THE BIRTH OF MODERN SCIENCE

IN the year 1543 Copernicus gave to the world the book which was destined to start a new epoch in the history of thought.

There is something symbolic in the fact that he received the first copy on his death-bed, for despite the tremendous consequences of his work, Copernicus himself belongs much more essentially to the age which was passing than to that which he heralded. The main element of the philosophy of the time was the conviction that the one necessary and sufficient source of intellectual truth resided in the recorded works of Aristotle. The contribution to thought of the age itself was chiefly confined to the exposition and interpretation of the Aristotelian philosophy, and of its characteristics two have a special interest for us. First of all, there was the assumption of *general principles* according to which Nature operates. For example, it was taken as a dogma that the heavens were "ingenerable, incorruptible, unalterable and unpassible," while the earth was the contrary of this in most respects. Again, everything in Nature had a discernible purpose, so that it was sufficient to show that a thing was useless in order to prove that it did not exist. Secondly—perhaps as a corollary of the general principle just mentioned—there was a widespread habit of inventing *new existences* to account for any phenomenon that could not be explained, the phenomenon thereafter being considered worthy of no further attention. "We . . . reduce the cause of these and other the like natural effects," says Simplicius, the Aristotelian, in Galileo's *Dialogues*, "to *Sympathy*, which is a certain agreement and mutual appetite which ariseth between things that are semblable to one another in qualities; as likewise on the contrary that hatred and enmity for which other things shun and abhor one another we call *Antipathy*." "And thus," replies Galileo through the mouth of Sagredus, "with these two words men come to render reasons of a great number of accidents and effects which we see not without admiration to be produced in nature. But this kind of philosophating seems to me to

have great sympathy with a certain way of Painting that a Friend of mine used, who writ upon the *Tele* or Canvass in chalk, here I will have the Fountain with *Diana* and her Nimphs, there certain Hariers, in this corner I will have a Hunts-man with the Head of a Stag, the rest shall be Lanes, Woods and Hills; and left the remainder for the Painter to set forth with Colours; and thus he perswaded himself that he had painted the Story of *Acteon*, when as he had contributed thereto nothing of his own more than the names."

These two elements in reasoning—the adoption of general principles and assumption of new existences—are, I think, recognisable as the ancestors of our modern methods of abstraction and hypothesis, and the modification which has been produced in them is the hallmark of the modern scientific movement. We shall see how this has come about, but for the moment our concern is with Copernicus and his times. Let us return to them.

The cosmogony of the sixteenth century was that of Aristotle and Ptolemy, according to which the spherical Earth was fixed at the centre of the Universe and the heavenly bodies moved round it in paths which were combinations (often very complicated) of circular motions. The extreme cumbrousness of this system had long been evident, but the almost incredible hold which it had over the minds of mediæval thinkers is well illustrated by the remark of Alphonso of Castile in the thirteenth century, that if he had been present at the Creation he could have given the Creator some good advice. It was easier to doubt the wisdom of God than that of Aristotle. Copernicus, however, through influences which we cannot pause to consider, made bold to question whether the Ptolemaic scheme was the inevitable way of interpreting the celestial motions. In accordance with the custom of the time, he looked for some ancient authority for a possible alternative. Had another cosmogony been proposed? It appeared that one had.

"When, therefore, I had long considered this uncertainty of traditional mathematics, it began to weary me that no more definite explanation of the movement of the world-machine established in our behalf by the best and most systematic builder of all, existed among the philosophers who had studied so exactly in other respects the minutest details in regard to the sphere. Wherefore I took upon myself the task of re-reading the books of all the philosophers which I could obtain, to seek out whether any one had ever conjectured

that the motions of the spheres of the universe were other than they supposed who taught mathematics in the schools. And I found first, that, according to Cicero, Nicetas had thought the earth was moved. Then later I discovered, according to Plutarch, that certain others had held the same opinion. . . .

"When from this, therefore, I had conceived its possibility, I myself also began to meditate upon the mobility of the earth. And although the opinion seemed absurd, yet because I knew the liberty had been accorded to others before me of imagining whatsoever circles they pleased to explain the phenomena of the stars, I thought I also might readily be allowed to experiment whether, supposing the earth to have some motion, stronger demonstrations than those of the others could be found as to the revolution of the celestial sphere.

"Thus, supposing these motions which I attribute to the earth later on in this book, I found at length by much and long observation, that if the motions of the other planets were added to the rotation of the earth and calculated as for the revolution of that planet, not only the phenomena of the others followed from this, but also it so bound together both the order and magnitude of all the planets and the spheres and the heaven itself, that in no single part could one thing be altered without confusion among the other parts and in all the universe. Hence for this reason in the course of this work I have followed this system. . . ."

It is perfectly clear from this and other passages of a like nature that Copernicus was concerned less with the distinction between truth and error than with that between simplicity and complexity; what he wanted was a simple geometrical representation of the phenomena. This was quite mediæval in principle. It was one of the fundamental notions of Aristotle, expressed in various ways, that Nature is simple. "Nature," he says, "does nothing in vain"; and "That is done in vain by many means which may equally well be done with fewer"; a body was to be regarded as having only one natural motion; and so on. Had Aristotle made no pronouncement on the movements of the heavenly bodies it is reasonable to suppose that the doctrine of Copernicus would have met with no great opposition. In more features than is commonly supposed it included Ptolemaic ideas. The universe was finite and spherical; the stars were all equidistant from the solar system, and the motions of the heavenly bodies were uniform and circular or were made up of combined circular motions. And all this, not because observation

required it, but because spheres and circles were the most perfect figures. It must be recognised, therefore, that Copernicus introduced no new element into philosophy; the beginnings of Science are not with him. His significance lies in the fact that by his new application of old principles he made it possible for the true successor of those principles to be recognised. He cleared the ground, but it is to Galileo that we owe the building.

In attempting to assess the importance of Galileo in the history of thought we must be more than usually on our guard against the influence of isolated remarks divorced from their context. It is possible to arrive at precisely the wrong conclusions unless we maintain a strict vigilance in this respect; of this we shall see an example presently. I would therefore make it clear that the quotations I shall give are introduced not as evidence but as examples of the general views here attributed to him. No just estimate of his colossal intellect can be made without a careful study of all the available evidence.

First and foremost, he opposed to reliance on the authority of Aristotle the maxim that Nature is made known to us only through observation; we have no instinctive or revealed knowledge of her processes. "Nature," we have already heard him say, "nothing careth whether her abstruse reasons and methods of operating be, or be not exposed to the Capacity of Men." The prime business, then, of the philosophy of Nature was observation, in comparison with which reasoning and argumentation were idle wind. "I know very well," he says, "that one sole experiment, or concludent demonstration, produced on the contrary part, sufficeth to batter to the ground these and a thousand other probable Arguments."

The revolutionary character of this outlook is sufficiently obvious, yet it is perhaps not superfluous to note that its importance lies in deeds rather than in words. As a matter of formal doctrine the trustworthiness of observation was as thoroughly Aristotelian as it was Galilean. The appeal to sense was, in fact, urged on behalf of Ptolemy against Copernicus. It is Simplicius the Aristotelian who says, "If *Copernicus* his opinion be imbraced, the *Criterium* of natural Philosophy will be, if not wholly subverted, yet at least extreamly shaken. Which, according to the opinion of all the sects of Philosophers requireth, that Sense and Experience be our guides in philosophating." And again, "*Aristotle*, though of a very perspicacious wit, would not strain it further than needed: holding in all his argumentations, that sensible experiments were to be preferred before any reasons founded

upon strength of wit, and said those which should deny the testimony of sense deserved to be punished with the loss of that sense." And again, in desperation, "But for God's sake, if it move transversely, how is it that I behold it to move directly and perpendicularly? This is no better than the denial of manifest sense; and if we may not believe sense, at what other door shall we enter into disquisitions of Philosophy?" On the other hand, it is Salviatus, representing Galileo, who again and again expresses his astonished admiration that Aristarchus and Copernicus could arrive at the truth by reasoning when the sensible evidence was incomplete. "I cannot find any bounds for my admiration, how that reason was able in *Aristarchus* and *Copernicus*, to commit such a rape upon their Sences, as in despight thereof, to make her self mistress of their credulity."

It is easy, therefore, to miss the whole significance of the Galilean revolution if we rely only on textual authority. It is when we turn to facts that the truth becomes manifest. The appeal to sense was doubtless urged by Aristotle and accepted by his self-styled representatives, but it was accepted only as a dogma; the idea that they should use their own senses in the investigation of Nature never occurred to them as a serious possibility. Thus, in support of the contention that if the Earth rotated all free objects on its surface would appear to move towards the west, they pointed out that if a body is let fall from the top of the mast of a ship under sail, it will strike the deck towards the stern because of the forward movement of the ship during the time of fall. This undoubtedly has the character of an argument from sense observation, but unfortunately its inventors omitted to try the experiment, relying on their idea of what would happen and not on the evidence of their senses at all. Galileo, on the other hand, by actual trial found that the body fell at the foot of the mast just as though the ship were not moving. The principal professor of philosophy at Padua, again, was so good an Aristotelian that he consistently refused to look through Galileo's telescope lest the evidence of his senses should force him to admit the existence of Jupiter's satellites and the mountains of the Moon. He contented himself instead with arguing them out of the sky. The primary innovation, therefore, which was introduced by Galileo was the direct appeal to Nature with a mind free from preconceptions of what her answer would be.

The contrast between Galileo and Copernicus on this point is very striking. In discussing the diurnal motion, Galileo, like Copernicus,

urges the greater simplicity of attributing it to the Earth, and he does so more thoroughly, but he is careful to add that such considerations do no more than make it *probable* that it is the Earth that moves: the *truth* of the matter is an independent question which can be answered only by observation. Waiting, therefore, until he has demonstrated, from the varying magnitudes of the planets (which Copernicus scarcely considered at all), that the *annual* motion belongs to the Earth and not the Sun, he then deduces that the Earth must rotate on its axis, or the day and night would each be six months in length. The difference in the attitudes of the two men towards this problem is a measure of the change which came with Galileo.

But this was not the only change. Not only was Nature to be interrogated by the senses, but also the evidence of the senses was to be interpreted mathematically as far as possible. Of the two besetting sins of the Middle Ages, Galileo had no use for arbitrary hypotheses but he was willing to accept *a priori* general principles if they were geometrical. That Nature conformed to these principles, however, though undoubtedly a strong conviction with him, was apparently a generalisation from experience and not a postulate. "I incline rather to think," he says, "that Nature first made the things themselves, as she best liked, and afterwards framed the reason of men capable of conceiving (though not without great pains) some part of her secrets." Nature is simple, he holds, but this again is a deduction from experience. "In the investigation of naturally accelerated motion we were led, by hand as it were, *in following the habit and custom of Nature herself, in all her various other processes*, to employ only those means which are most common, simple, and easy." There is a striking passage in a discussion of the nature of heat in which he gives us an invaluable insight into his view of Nature. I will quote it at length:

"But first I want to propose some examination of that which we call heat, whose generally accepted notion comes very far from the truth if my serious doubts be correct, inasmuch as it is supposed to be a true accident, affection, and quality really residing in the thing which we perceive to be heated. Nevertheless I say, that indeed I feel myself impelled by the necessity, as soon as I conceive a piece of matter or corporeal substance, of conceiving that in its own nature it is bounded and figured in such and such a figure, that in relation to others it is large or small, that it is in this or that place, in this or that time, that it is in motion or remains at rest, that it touches or does not touch another body, that it is single, few, or

many; in short, by no imagination can a body be separated from such conditions: but that it must be white or red, bitter or sweet, sounding or mute, of a pleasant or unpleasant odour, I do not perceive my mind forced to acknowledge it necessarily accompanied by such conditions; so if the senses were not the escorts, perhaps the reason or the imagination by itself would never have arrived at them. Hence I think that these tastes, odours, colours, etc., on the side of the object in which they seem to exist, are nothing else than mere names, but hold their residence solely in the sensitive body; so that if the animal were removed, every such quality would be abolished and annihilated. Nevertheless, as soon as we have imposed names on them, particular and different from those of the other primary and real accidents, we induce ourselves to believe that they also exist just as truly and really as the latter. I think that by an illustration I can explain my meaning more clearly. I pass a hand, first over a marble statue, then over a living man. Concerning all the effects which come from the hand, as regards the hand itself, they are the same whether on the one or on the other object—that is, these primary accidents, namely, motion and touch (for we call them by no other names)—but the animate body which suffers that operation feels various affections according to the different parts touched, and if the sole of the foot, the kneecap, or the armpit be touched, it perceives besides the common sense of touch, another affection, to which we have given a particular name, calling it tickling. Now this affection is all ours, and does not belong to the hand at all. And it seems to me that they would greatly err who should say that the hand, besides motion and touch, possessed in itself another faculty different from those, namely the tickling faculty; so that tickling would be an accident that exists in it. A piece of paper, or a feather, lightly rubbed on whatever part of our body you wish, performs, as regards itself, everywhere the same operation, that is, movement and touch; but in us, if touched between the eyes, on the nose, and under the nostrils, it excites an almost intolerable tickling, though elsewhere it can hardly be felt at all. Now this tickling is all in us, and not in the feather, and if the animate and sensitive body be removed, it is nothing more than a mere name. Of precisely a similar and not greater existence do I believe these various qualities to be possessed, which are attributed to natural bodies, such as tastes, odours, colours, and others."

This is an early statement of the now familiar conceptions of

primary and secondary qualities. The primary qualities are not those to which we should give that title now, but the striking thing for us to notice is that they are all mathematical in character, whereas the secondary qualities, in Galileo's day at least, were non-mathematical. The whole of objective Nature, therefore, was in Galileo's view susceptible of mathematical interpretation. We might, of course, find the accomplishment of such an interpretation often beyond our powers, but that is another matter. The external world was potentially mathematical—that is the essential point.

The significance of this conception is perhaps insufficiently realised. "The limitation of the scope of physics to pointer readings and the like," says Sir Arthur Eddington,* "is the outcome of a tendency discernible far back in the last century but only formulated comprehensively with the advent of the relativity theory." But Galileo not only limited "physics" to "pointer readings and the like"; but whatever was not of this character was to him not a part of Nature at all, but a subjective quality. Eddington, in distinguishing in the external world the metrical from the non-metrical elements, is less far-reaching than Galileo, who allowed of no non-metrical elements at all. What has come about in later years is that through the introduction of hypotheses, scientific investigation has extended beyond the realm of Galileo's primary qualities, and hence has appeared to have potentialities in the non-metrical world. The significance of recent developments to which Eddington refers is that these extensions have themselves been found to be metrical. Those, therefore, who to-day hold that Science is limited to what can be measured, have not gone beyond Galileo in their characterisation of the physical world, but have simply found that that world is more comprehensive than he conceived it to be.

The essential features of modern Science are implicit in Galileo's view of Nature. The transition from Copernicus to Galileo is the transition from the mediæval to the modern. All that has followed is the natural evolution of the new idea which Galileo introduced into the world of thought.

* *The Nature of the Physical World*, p. 254.

CHAPTER II

NEWTON—THE MATURITY OF PHYSICS

GALILEO'S work was brought to full fruition by Newton, who entered this world in the year in which Galileo left it. With Newton, the idea that we have no *a priori* knowledge of Nature reached its extremist form. We must again notice, however, the danger of isolated quotation, which is even more hazardous with Newton than with Galileo. Newton does not hesitate to generalise tentatively from observation —indeed, such generalisation constituted his great contribution to Science—and his generalisations, expressed without the qualifying requirement that they are always subject to the evidence of experiment and observation, look perilously like the arbitrary principles of the Middle Ages. Consider, for example, the dogma that Nature is simple, which we have already noticed. This was accepted by Newton, but not because it was self-evident: like Galileo, Newton accepted it because he had found it to hold and considered himself justified in assuming it to be true universally until evidence to the contrary was forthcoming. This is not always recognised, and Newton is sometimes misrepresented by the quotation, apart from the context, of the first two of the "Rules of Reasoning in Philosophy," given in the *Principia*, viz.: "*We are to admit no more causes of natural things, than such as are both true and sufficient to explain their appearances.* To this purpose the philosophers say, that Nature do's nothing in vain, and more is in vain, when less will serve; for Nature is pleas'd with simplicity, and affects not the pomp of superfluous causes. *Therefore to the same natural effects we must, as far as possible, assign the same causes.*" This principle, familiarly known as "Occam's razor," is completely analogous to the principle on which Copernicus declared the planets to move in circular orbits. Nature was perfect; a circle was the most perfect figure; therefore the heavenly bodies moved in circles. Similarly—Nature is simple; one cause is more simple than two; therefore the same natural effects have the same causes. The fallacy of the argument, if not immediately obvious, can be seen by the most homely of examples. If I meet a man with a broken arm, and shortly afterwards meet another man with a black eye, the single assumption that the two men have been fighting will explain both effects, but clearly it is not necessarily, or even probably, the truth.

Occam's razor is not a safety razor in practice. Of course, if the whole of natural phenomena be taken into account it may be that the group of causes which is actual will be found to be the simplest and most economical, but if that opinion be held it is held purely as a matter of faith, and in any case a rule of reasoning in philosophy which is applicable only when it is no longer required is hardly worth insisting upon.

We must not, however, saddle Newton with such an opinion, as we are naturally led to do when we read the first two rules of reasoning alone. We must remember the fourth rule, which provides the indispensable qualification: "*In experimental philosophy we are to look upon propositions collected by general induction from phænomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phænomena occur, by which they may either be made more accurate, or liable to exceptions.* This rule we must follow that the argument of induction may not be evaded by hypotheses." We shall find no consistency in Newton's practice or doctrine unless we realise that, in his view, reasoning must both begin and end in observation.

In the matter of fitting observations into a mathematical framework, Newton was both more and less thoroughgoing than Galileo. He himself enlarged the framework considerably, so that while to Galileo mathematics was mainly geometry, to Newton geometry occupied only a subordinate place. Thus he was able to conduct a mathematical treatment of the phenomena of colour which Galileo had relegated to the rank of a subjective quality. On the other hand, he did not regard the whole of external Nature as necessarily mathematical in character, although he hoped it might prove to be so. "For," he says in the Preface to the *Principia*, "all the difficulty of philosophy seems to consist in this, from the phænomena of motions to investigate the forces of Nature, and then from these forces to demonstrate the other phænomena. And, to this end, the general propositions in the first and second book are directed. In the third book we give an example of this in the explication of the System of the World. For by the propositions mathematically demonstrated in the first books, we there derive from the celestial phænomena, the forces of Gravity with which bodies tend to the Sun and the several Planets. Then from these forces by other propositions, which are also mathematical, we deduce the motions of the Planets, the Comets, the Moon, and the Sea. I wish we could derive the rest of the phæ-

nomena of Nature by the same kind of reasoning from mechanical principles. For I am induced by many reasons to suspect that they may all depend upon certain forces by which the particles of bodies, by some causes hitherto unknown, are either mutually impelled towards each other and cohere in regular figures, or are repelled and recede from each other; which forces being unknown, Philosophers have hitherto attempted the search of Nature in vain. But I hope the principles here laid down will afford some light either to that, or some truer, method of Philosophy."

Thus, while Newton was inclined to believe that Nature was essentially and entirely mathematical, he was willing to concede the possibility of some truer philosophy. The same recognition of wider possibilities is shown at the end of the *Principia*, where he tells us that to discourse of God "from the appearances of things, does certainly belong to Natural Philosophy," and he thinks it not superfluous to say that "bodies find no resistance from the omnipresence of God."

But the real significance of Newton in the history of Science is not in his view of the scope of mathematics in philosophy, but in his achievement in constructing a mathematical system of the world which has been our almost unquestioned guide for more than 200 years. Let us look for a moment at the first two of Newton's famous laws of motion:* "(1) Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impress'd thereon. (2) The alteration of motion is ever proportional to the motive force impress'd; and is made in the direction of the right line in which that force is impress'd." Taking these laws in conjunction with the definitions which precede them, and remembering that Newton's whole object is the study of phenomena and not the erection of an *a priori* philosophy, we see that these two laws are inseparable. The first law, for example, cannot possibly be tested for, according to Newton's own philosophy, we can never observe the motion of a body free from the impressed force of gravitation at least. Even if we could escape from all known forces, then any failure of a body to move rectilinearly and uniformly could at once be ascribed to an unknown force, and the law cannot, therefore, even conceivably be proved or disproved: standing by itself it is an entirely

* There is also a third law, relating to stress, but since our purpose is only to illustrate general principles, we need not consider it here. This law, like the others, has lately been absorbed into the general scheme of relativity.

arbitrary principle. The term "force" is consequently the name we give to a conceptual cause of an observed change of motion; it is the nominative of the verb, "to accelerate." Force has no right to exist unless the first law be admitted, and the first law cannot be tested because of its possible existence. The two laws therefore must be taken together as a particular way of describing observed motions. We divide the motion of a body into a passive part belonging to the body and an active part belonging to a force, and the combination of the two leads to the observed motion. It is like saying that every number has an innate tendency to be unity, which is frustrated by the interference of an "excess." Thus, 3 would be unity by Nature, but the excess, 2, gives it its actual value.

What is the justification for this apparently arbitrary procedure? Only that it allows the main phenomena of Nature to be accurately represented very simply. If every motion had been assumed to be indivisible, no accurate generalisation would have been possible. Aristotle had tried that device and failed. If, again, the "natural" motion of bodies had been assumed to be circular instead of rectilinear, all the motions observed in Nature could have been described in terms of disturbing forces, but those forces would have been susceptible of formulation only by exceedingly complicated expressions. The rule of the simplicity of Nature would have been broken, and this rule is absolute when phenomena are not violated. The feature of Newton's scheme which redeemed it from absolute arbitrariness was that it made possible a simple, general, and accurate account of phenomena.

The partially arbitrary character of the scheme was, of course, perfectly recognisable in Newton's day, and he doubtless recognised it to the full. Possibly it had something to do with his consistent refusal to inquire into the cause of gravitation, although there is no doubt that he believed that gravitation had a real cause which might one day be discovered. For the scientist, however, whatever objections the philosopher might have, there is nothing in this arbitrariness to invalidate the wholehearted adoption of the scheme. The mathematical physicist, as someone has said with reference to the equations of electro-magnetism, spends his time dividing nothing into two parts, and if he is willing to do this he will not object to a similar division of something, e.g. the motions of bodies. He does not care how arbitrary the division might be so long as it satisfies two demands —first, that he is given a rigid criterion for making it; and, second,

that it leads to a simple correlation of phenomena without implying anything contrary to observation. The second condition was completely satisfied in Newton's time and almost up to our own; the first was not, so he proceeded to fulfil it. He defined absolute space and absolute time, so that accelerated motion could be unambiguously defined.

We shall see later that it is this last procedure that has led to the downfall of Newton's scheme. Absolute space and time appear to be inherently inaccessible through observation, so that the measurement of acceleration remains an unrealisable ideal. We will ignore that for the moment, however, and consider Newton's scheme as he left it, and the point which is most important for our purpose is that it is essentially a development of the ancient habit of setting up principles according to which Nature works. The laws of motion and gravitation were statements of such principles, which perhaps in time all the phenomena of Nature might be found to exemplify. But these laws differ from the principles of the Middle Ages—such as those expressing the perfection and simplicity of Nature and the idea that everything in Nature is designed to serve man—first, in that they are capable of strict mathematical definition, and secondly, in that they are generalisations from every relevant observation and are entirely subject to the approval of later observations. In other words, they are mathematical abstractions from observations. The terms in which they are expressed are none of them observed facts, but all are abstracted from facts. Mass is the measure of the arbitrarily assumed resistance of bodies to change of motion: force is the arbitrarily assumed agency which makes bodies depart from the arbitrarily assumed natural state; gravitation is a particular force which appears to be universal in its operation; acceleration is the rate of change of speed measured in terms of an arbitrarily assumed absolute space and time. All these things are purely conceptual, yet the laws of motion and gravitation made out of them described the motions of experience with perfect fidelity, and correlated with one another the motions of bodies on the Earth and in the Heavens—an achievement justly regarded as the greatest in the history of Science.

Of the two methods of correlation employed by Science at the present time, therefore, Newton was the supreme exponent of one—the method of abstraction. Of the second—the method of hypothesis—he was the most uncompromising of opponents. He accepted the

traditional practice of enunciating principles of Nature because, in the rigid alphabet of mathematics, he could formulate principles which were indissolubly wedded to phenomena. But he could not do this with the other traditional practice, that of formulating hypotheses, for a hypothesis was but one out of many possible guesses and there was no means of knowing which of them was the true one. "In the meanwhile give me leave to insinuate, Sir," he wrote, "that I cannot think it effectual for determining truth, to examine the several ways by which phenomena may be explained, unless where there can be a perfect enumeration of all those ways." And in the *Opticks* he writes: "These Principles [gravitation, cohesion, etc.] I consider, not as occult Qualities, supposed to result from the specifick Forms of Things, but as general Laws of Nature, by which the Things themselves are form'd; their Truth appearing to us by Phænomena, though their Causes be not yet discover'd. For these are manifest Qualities, and their Causes only are occult. And the *Aristotelians* gave the Name of occult Qualities, not to manifest Qualities, but to such Qualities only as they supposed to lie hid in Bodies, and to be the unknown Causes of manifest Effects: Such as would be the Causes of Gravity, and of magnetick and electrick Attractions, and of Fermentations, if we should suppose that these Forces or Actions arose from Qualities unknown to us, and uncapable of being discovered and made manifest. Such occult Qualities put a stop to the Improvement of natural Philosophy, and therefore of late Years have been rejected." He would not allow his speculations on the nature of light to be considered as a hypothesis, and although he adopted a kind of atomic theory, we find on examination that it went not one step beyond observation. His ultimate particles had all the properties of those with which we are familiar except their relatively great size and (although he expresses his willingness to abandon this on evidence being produced) their liability to fracture. The idea had nothing essential in common with the modern atomic theory.

CHAPTER III

DEVELOPMENTS AFTER NEWTON—ABSTRACTION AND HYPOTHESIS

THE great influence of Newton dominated the activities of physicists and mathematicians during the eighteenth century, and the progress of that time is summed up partly in the successful application of his principles to detailed phenomena and partly in the reformulation of his essential ideas into alternative expressions more convenient for certain calculations. Hypotheses were at a discount. But, fortunately for Science, other influences were independently at work which, so far as physical Science is concerned, found the most effective medium of operation in chemistry. Chemistry, no less than physics, had throughout the Middle Ages been held back by the arbitrary adoption of principles and occult qualities, but it had suffered no martyrdom such as had canonised physics and astronomy in the person of Galileo. It had therefore no need of a formal body of doctrine, and while it could profit by the purifying influence of the great physicists, it could also regard with tolerance what to them was heresy. Consequently we find chemistry freeing itself from superstition more gradually than physics, but at a smaller cost. While Galileo was compressing the whole of external Nature into a geometrical category, Boyle was about to declare it knowable only by experiment without troubling to make the knowledge mathematically definite; and while Newton was declaiming against hypotheses, Stahl was conjuring up the spirit of phlogiston which was to direct chemical progress for the next hundred years. This freedom of chemistry to be influenced but not controlled by the mathematical revolution is, I think, of the utmost importance, for by the gradual merging of the two sciences into one, physics has slowly become emancipated from the restrictions which Newton imposed, although whether she has done so without sacrificing the principles for the sake of which he imposed them is a matter of grave doubt which we must consider at length later. We may pause for a moment, however, to notice that even in chemistry there have not been wanting rigid adherents to the Newtonian philosophy. When Lavoisier placed chemistry on a quantitative basis at the end of the eighteenth century, he did something analogous to what Newton did for physics a hundred years

earlier, and Wurtz, in his history of chemistry, speaks of that subject as having originated with Lavoisier. Remembering Priestley and Cavendish, to name no others, we shall not be likely to accept this, but it is a sign that the spirit of the strict mathematical philosophy of Nature, which chooses only abstractions for its incarnation, was not excluded from the chemical world.

It is impossible here to trace in detail the introduction of hypotheses into physics, nor indeed am I qualified to do so; it is a matter for the historian. To illustrate the process, however, I will give three quotations from representative scientists, covering the period from Newton to the present time and separated by roughly equal intervals. The first is from Newton himself (1687): "I frame no hypotheses. For whatever is not deduc'd from the phænomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy." The second is from Laplace, referring to his famous "nebular hypothesis" (1796): "I will suggest an hypothesis which appears to me to result with a great degree of probability, from the preceding phenomena, which, however, I present with that diffidence, which ought always to attach to whatever is not the result of observation and computation." The third is from Eddington (1926): "Care is taken to provide 'macroscopic' equations for the human scale of appreciation of phenomena as well as 'microscopic' equations for the microbe. But there is a difference in the attitude of the physicist towards these results; for him the macroscopic equations —the large-scale results—are just useful tools for scientific and practical progress; the microscopic view contains the real truth as to what is actually occurring." The course of development is from a categorical rejection of hypotheses of any kind whatever, through a diffident presentation of one which results "with a great degree of probability" from phenomena, to the confident assertion that a hypothesis contains "real truth" and phenomena are just "useful tools." The question of the validity of this process is the most vital question, both for the philosophy of Science and for the application of scientific ideas to other departments of thought, at the present time.

I shall not trace the history of physical Science through the nineteenth century in any detail, but I must point out that it has followed the two courses of abstraction and hypothesis, between which a close correspondence has been found to exist. Thus, Newton's great

scheme of abstraction has been extended to cover electric and magnetic phenomena. Certain accelerated motions exhibited by bodies have been ascribed to electric and magnetic forces, and just as gravitation was measured in terms of a conceptual "mass," these forces are measured in terms of a conceptual "electric charge" and "magnetic pole." Other abstractions, not quite so slavishly copied from Newton's process of thought but altogether in keeping with his fundamental attitude to Nature, are embodied in the principles of the conservation of energy and the second law of thermodynamics. Here the conceptions of energy and entropy, every whit as transcendental as the conception of mass, have been abstracted from observations and used as the terms in which universal principles are expressed. These are definite steps towards the realisation of Newton's ideal of deriving "the rest of the phænomena of Nature by the same kind of reasoning from mechanical principles."

But side by side with this structure of thought has gone the building up of the great edifice of atomic physics, based on a hypothesis derived from chemistry. Chemists, as we have seen, were for historical reasons less restricted than physicists in their methods of investigation, and the placing of the atomic theory, not merely on a scientific, but even on a numerical basis by Dalton early in the nineteenth century, made available a hypothesis peculiarly tempting to mathematical physicists. The bonds of tradition (witness the attitude of Laplace, mentioned above) had become loose. Hypotheses, at first looked at askance, soon became favoured by more direct glances, and the inevitable happened. The seductions of Dalton's conception proved irresistible and physics fell: the kinetic theory of gases became a part of its structure.

Before proceeding to the developments of recent times, it is well that we should inquire what status hypotheses hold in Science, for a very large part of the philosophical and general significance we shall attach to modern physics depends on our answer to this question. I have already mentioned that hypotheses may be of two kinds: namely, those potentially verifiable by observation and those inherently beyond such verification. The assessment of the first kind offers no difficulty. If the hypothesis is verified it becomes a fact, and those common experiences of which our definition of Science treats become thereby enriched. If it is not verified, under conditions of observation which from its nature should give a categorical answer to the question of its actuality, it must be either rejected or

retained only as a convenient parameter for the co-ordination of the limited field of experience in which it grew. If, for practical reasons, a test of its actuality is not immediately possible, it may still be retained for convenience, and the question of its complete establishment must be held in abeyance.

All this is obvious enough, although it is perhaps not always followed with strict fidelity. The real problem arises when we consider hypotheses which, because of their essential qualities, do not admit of test by observation. There are two classes of these. First, there are hypotheses—such as that mentioned in the Introduction, which has been advanced to explain the origin of the solar system—which cannot be verified because they postulate happenings in the past. Those happenings need not be unobservable because of their essential character; in practice they never are. They are unobservable simply because the opportunity for observing them has gone by. To this class belong all theories of evolution and all the events of history which are not unquestionably attested. It is very difficult to know how to regard hypotheses of this kind. We can certainly use them as convenient tools, but the question whether the postulated events actually happened can be answered only in terms of probability—not the definite numerical probability familiar to physicists and mathematicians, but the vague quality which depends on individual predisposition almost as much as on the concrete evidence. The difficulty is particularly acute for those who hold some form of idealistic philosophy. If the world of sensation exists only in consciousness, what status are we to assign to events assumed to have taken place before consciousness was?

Happily Science is not called upon to answer this question, for in every case in which there is necessary doubt about the actuality of a hypothesis the actuality has no scientific importance. Science is concerned with experiences, and if an event is outside experience, for whatever cause, it can enter Science only as a tool. As biologists it does not matter to us whether the higher forms of life actually emerged from the lower ones or not. The hypothesis that they did so emerge correlates our observations as well as a conception as it would do as a reality, and we can therefore employ it so long as it continues to be of value. The question may be tremendously important to us as moralists say, but in that case it is as moralists that we must answer it. The same thinkers are often both scientists and moralists, and much of the misunderstanding of the past has

arisen from the fact that they have confused their two attitudes to the universe. It is no doubt galling to the scientist to be told, of a brilliant and beautiful hypothesis, that it is only a guess because it lacks a verification which from the nature of things is impossible, but it is only his human, not his scientific self that can be so galled. Scientifically he is just as satisfied with a guess as with a "fact" if they are beyond observation, and he can receive the taunt of the critic with indifference.

But there is a second class of unverifiable hypotheses; namely, those which, being independent of time, postulate existences or events whose unobservability is part of their essential nature. The great discovery of the last few years, as we shall see, is that the whole microscopic scheme of physics belongs to this class. Our ideas as to its status must lie at the foundation of whatever philosophical structure we raise in the light of modern physical Science. What are we to say, then, of hypotheses such as these?

I think the conclusion is inescapable that the existences which they postulate (*e.g.*, electrons and protons) can be regarded only as concepts, possessing no properties and subject to no laws other than those which are necessary and sufficient to enable them to correlate observations. The question whether they are *real* or not is already answered in the designation of them, for we can hardly regard anything as real, in the sense in which observed existences are real, which is essentially unobservable. If we adopt such hypotheses, therefore, we have perfect freedom to shape them as we please, provided they achieve the end for which they were created. We are not bound to give them the characteristics of phenomena. They are employed for rational correlation and are not admissible to experience: they must therefore have rational properties, but not necessarily sensible ones.

I have called this conclusion "inescapable." Perhaps it may be objected that a thing may really exist although it is unobservable, and that what we have to do is to find out what it is like and not make it what we please. This objection would not be held valid by modern physicists, but I do not wish at this stage to invoke their reasons for this attitude; we can deal with the matter on less general grounds. Let it be granted, for the sake of argument, that these unverifiable hypotheses are actual existences whose nature we have to discover. We can discover it only by inference from observation of phenomena, for the existences themselves are unobservable. Now

it will be admitted that their nature is not *necessarily* that of phenomena, so that in making our inferences we must not restrict ourselves to characteristics which would be possible for observable bodies. In other words, we are free to infer anything about these hypothetical existences, provided it is an inference from observation. Now we can test the validity of our inferences only by deducing all their observable consequences and checking the results by actual observation. If they pass the test, our inferences have all the justification possible to them. But now, it is conceivable that they may do this and yet not be the only set of inferences which would give the same results. Hence, if the hypothetical existences are actual, all we can ever find out about their nature by methods possible to Science is an indefinite number of possible alternatives: no criterion exists for distinguishing between these alternatives. It follows, therefore, that whether the existences are real or not, our characterisation of them must be purely ideal, and since our characterisation of them is all that we can ever mean when we discuss them, we are compelled to regard them only as concepts, with no necessary likeness to phenomena and no obligation to obey any laws but those by which they issue in phenomena.

The implications of this conclusion are so important that we will venture to elaborate them to what may seem an excessive degree. They constitute, in fact, as it seems to me, the most fundamental element in the evolution of Science during the last hundred years. Although they have so far affected only physical science, in a way which I hope to describe, they are of universal significance, and sooner or later the biological and psychological sciences will doubtless avail themselves of the wider freedom they afford. Briefly, the position is this. These essentially unverifiable hypotheses constitute a new instrument for scientific correlation. They are inherently different from those hypotheses of the more familiar type which are of the nature of potential experiences. Let me call them "correlates" for the moment, to make this distinction as sharp as possible. Then we may say that the instruments of Science are of three main types, comprising abstractions, hypotheses, and correlates. The old question, "Is the hypothesis true?" then has a definite meaning, though we may not always be able to answer it, but the corresponding question, "Is the correlate true?" is meaningless. It makes its nearest possible approach to a meaning when we put it in the form, "Does the correlate correlate?" The correlate is true only in so far as it does so, but

then it exists only in so far as it does so. A correlate is a pure mental edifice, of which the bricks are any ideas we care to form and the cement any conditions we care to impose. The only restrictions on our freedom are that the conditions must be rational and the building must accommodate experiences. It is unnecessary to give an example of a correlate here, for a large part of this book is a description of one; namely, the atomic hypothesis.

The distinction between hypotheses and correlates has only lately been realised. There are two things to be said about it, both of which are of great importance. The first is that the recognition that correlates are as valid weapons as hypotheses gives the scientist an enormous increase of power. He is no longer restricted in his thinking to elements which can be imagined and pictured, to elements which are clothed in the characteristics of phenomena. He may use any elements at all provided that he can define them rationally, and he may suppose them to interact with one another in any conceivable way provided that he can represent their behaviour by rational statements. This is legitimate because the aim of Science is to give a *rational* correlation of phenomena, but not necessarily an *imaginable* one. The second point is that since no increase of power can be obtained without a concomitant increase of responsibility and liability to error, we must be strictly on our guard, when employing correlates, to remember that they are correlates and not hypotheses. That is the condition on which the employment of correlates is legitimate. It will not do to claim the liberty of using any elements of thought for the purpose of correlation and then, when we find that they succeed in their purpose, to conclude that they must therefore be "true" or "real" or whatever other fine name we like to bestow on them. They are instruments of correlation, and their success means that so far as it extends, Nature is rational—or, if subjective terms are preferred, our experiences are consistent with our reason. This itself is a great achievement: I think many would regard it as of far greater import than the attachment of the label "true" or "real" to the network by which experiences are held together. But to others the proper distribution of those labels is the ultimate aim of life, and it cannot be too clearly realised by them that correlates and experiences must not in any circumstances be given identical labels.

I hope the word "correlate" has now served its purpose and therefore—since I do not wish to incur the responsibility of coining a new word, or, rather, imposing too restricted a meaning on an old one—

I will cease to use it, regarding "correlates" simply as hypotheses which are essentially unverifiable. The word "hypothesis," therefore, will henceforth have a wider meaning than that which I have just been assigning to it; it will include correlates as well. In scientific practice this has the advantage of leaving the scientist free to frame a hypothesis without having to decide at the beginning whether it is a hypothesis in the restricted sense or a correlate; only he will have to bear in mind the possibility that it may be either.

Let us return now to the development of physics in the nineteenth century. We had reached the point at which physics adopted the hypothesis of atoms. The significance of this innovation is worthy of close attention. The hypothetical atoms (or molecules, as they were at first called), unlike those conceived by Newton, were not only unobserved, but were probably unobservable. Hence the only properties which could properly be assigned to them were such as were necessary to make them capable of correlating observations. So long as this was acknowledged it mattered not whether they were regarded as convenient fictions or as possessing the same kind of reality as gross bodies. There is no doubt that at least the great majority of physicists favoured the second alternative, and it was perhaps on this account that the atoms were almost instinctively endowed, as if by natural right, with the familiar conceptions abstracted from the behaviour of observed bodies. They were assigned mass and, later, electric and magnetic charges. They were supposed to move in absolute space and time and to be subject to the same forces that had proved useful in the description of observed motions.

Now this, as we have seen, was not at all necessary. For example, it was no doubt simple, and therefore tentatively justifiable, to suppose that the mass of a body was the sum of the masses of its atoms, but it was not justifiable to suppose that this was inevitably so. Mass, being an abstraction from the behaviour of gross bodies, had not necessarily any meaning in relation to a single atom. Its emergence when matter was considered in bulk might have been just as validly ascribed to the *association* of the various atoms with one another as to their individual properties. The *volume* of a gas, in fact, was so assigned, and it is interesting to recall that chemists, less impressed than physicists by the universality of Newton's abstractions, had not hesitated to endow their "phlogiston" with a negative weight rather than give it up. In the light of modern knowledge, it is clear that in this respect they had truer ideas of the pos-

sibilities of hypotheses than the physicists of a later generation, who unconsciously repeated the error of mediæval thinkers. For, just as the Aristotelians accepted Aristotle's doctrine that the testimony of sense should be regarded, but nevertheless refused to heed such testimony, so the nineteenth-century physicists adopted Newton's abstractions without realising that Newton would have held them valid only over the range of observation. Thus the danger which Newton dreaded crept unseen into physics, and it has been left to our own time to discover and expel it at an incalculable cost to our sense of the intelligibility of things.

Nevertheless, the hypothesis could claim credit for immediate advances of remarkable rapidity and range. It would be superfluous to recount them here, but one feature of the process which demands notice is the parallelism which was maintained at many points between the abstractions from observation and the elaborations of the atomic hypothesis. One of the most interesting examples of this is provided by the second law of thermodynamics. This was originally formulated simply as a statement of the universal experience that on the whole the tendency of natural processes is irreversibly in a single direction, which may be expressed as the direction towards uniform level.

> Even the weariest river
> Winds somewhere safe to sea.

In finite regions a difference of level might be created or increased; thus the radiation of the Sun transfers the water of the sea back to the river-source again. But in all such cases a greater (or, at best, equal) movement towards uniformity elsewhere accompanies the process. Sunlight cannot return the river to its source without reducing the difference of potential energy between itself and the Earth by a more than compensating amount. The second law of thermodynamics is thus an abstraction from the whole of physical experience that has yet been passed through: it is essentially independent of any hypothesis. Nevertheless, it has been formulated also in terms of the atomic hypothesis with, so far as we can see, complete universality of application. The irreversible tendency of Nature is interpreted as a change from organised to random motions of atoms, and this conception gives a description of events which runs *pari passu* with that of the simple abstraction.

Another example is provided by the famous Michelson–Morley experiment. Here the original interpretation was expressed in terms

of the amplified atomic hypothesis, and is familiarly known as the "Fitzgerald–Lorentz contraction." Only afterwards was an interpretation given in terms of pure abstraction, but again the parallelism is complete.

The real value of the atomic hypothesis, however, is not that it provides an alternative to more direct processes, but that it correlates phenomena whose distinctive features are too detailed to be noticed by the frigid austerity of the great abstractions. The laws of gravitation, electro-magnetism, and the like recognise no relation between the characteristic properties of one body and those of another; even laborious experiment and observation by themselves cannot reduce the number of elementary bodies to less than 90 or thereabouts. But the atomic hypothesis in its later developments can express these 90 elements in terms of only two independent particles, the proton and the electron, and there is promise of these becoming related with one another. Abstractions serve to correlate the general qualities of our experiences, but hypotheses must be employed to correlate the particular qualities •

CHAPTER IV

THE EXTREMITY OF ABSTRACTION—THE THEORY OF RELATIVITY

At the beginning of the twentieth century, then, we find physics with a vast accumulation of data—of common experiences—many of which had been satisfactorily correlated with one another through the medium of the great abstractions (such as the laws of motion, gravitation, electro-magnetism and thermodynamics) and the great atomic and electronic hypothesis. Each of these instruments of correlation had been developed with magnificent success and seemed capable of meeting almost all requirements; only a very few experiences remained obstinately intractable. These experiences, however, have proved able profoundly to modify the whole double-sided scheme of physics. The Newtonian abstractions have had to yield to those of relativity, and the atomic hypothesis has been transformed by the advent of the quantum theory. We will consider the nature of these modifications in turn.

I do not propose to give a formal account of the theory of relativity. That would be unnecessary for those already acquainted with it, and those who are not are doubtless sufficiently familiar with the phrases in which it is usually explained to make a repetition of them superfluous. Moreover, the details of the theory are not needed for our purpose: all that we are concerned with is the change of outlook which has led to its adoption.

We have seen that Newton's laws of motion are intelligible only if accelerated motion can be definitely distinguished from rest or uniform motion. Newton provided for this by postulating an absolute space and an absolute time in terms of which any type of motion could be unambiguously described. By "absolute" is meant complete homogeneousness and independence of all physical bodies or events. The Newtonian absolute space and time were like the Aristotelian heavens—ingenerable, incorruptible, unalterable, and unpassible, wholly indifferent to the events of Nature. What happened to-day would happen to-morrow; what happened here would happen there —provided the matter and forces of Nature were the same. Since a falling body moved farther in one second than in the previous second, a force must be acting on it; there could be no difference in

the motion arising from the mere difference of time. Similarly, since bodies moved differently at the Earth's surface and at the distance of the Moon, the force of gravitation must be different in the two places; there could be no difference in the motions arising from the mere difference of place. With such conceptions the velocity of a body would be uniquely measured by the amount of absolute space passed over in a unit of absolute time.

Now it did not matter greatly, from Newton's point of view, whether the absolute velocity of any body could be definitely stated: all that was necessary was that such velocity was definitely conceivable. Motions are always of necessity measured with respect to some chosen standard of rest. If that standard of rest should happen to be at rest in absolute space, then the measured motions would be the absolute ones. If the standard was moving uniformly in absolute space, then all the measured motions would be liable to correction by the same quantity, and the motions of the bodies among themselves would be unaltered. If, finally, the chosen standard of rest were moving non-uniformly in absolute space, then all the measured motions would still be similarly liable to correction and the motions of the bodies among themselves would still be unaltered, but the whole system of bodies would be subject to a force, undetectable so long as the motion of the standard of rest was unknown. In practice, therefore, it did not matter whether the absolute motion of any body was known or not. But what did matter was that the motion of one body with respect to another should be precisely assignable.

In Newton's absolute space and time this requirement was met. If two bodies moved with *absolute* velocities, U and V, in the same direction, then the velocity of one with respect to the other would be $U - V$. It followed that if two bodies moved with velocities U and V *relative to a third body*, the velocity of one with respect to the other would still be $U - V$. Hence all relative velocities were measurable, and therefore all relative accelerations and forces were detectable. The correlation of all observed motions could therefore be brought about without knowing the absolute motion of any body in the universe. The only essential purpose served by absolute space and time was that of giving a formula for the composition of observed motions. Space and time themselves were as ideal as the division of motion into a uniform and an accelerated part.

This definition of relative velocities was consistent with all experience obtained up to Newton's time, so there was no objection

to adopting it. The theory of relativity became necessary when it transpired that for very high speeds it ceased to be applicable. If two bodies move with measured speeds U and V relative to a third body, and one of these velocities is comparable with the velocity of light, the measured velocity of one body relative to the other is not $U-V$, but a more complicated expression. This is simply a deduction from experiment. Hence the fundamental requirement of Newton's philosophy—namely, that knowledge shall begin and end in experiment—requires us to abandon the assumption of absolute space and time. We must take account of the fact that the measured space and time intervals between events—and therefore also the measured velocities of bodies—are different for observers in relative motion, and in the absence of an absolute space and time there is no means of deciding which observer's measurements are right. Velocity and acceleration have therefore no absolute meaning, force has no absolute meaning, and the whole Newtonian scheme breaks down because it is incapable of correlating our *common* experiences. It is satisfactory so long as it is applied by a set of observers relatively at rest, but the forces which such observers deduce will not correlate the experiences of others who are moving with respect to them.

We can, however, again find common ground if we unite space and time into a single whole, regarding time as a fourth dimension of space. This again is found by experiment and observation, and experiment and observation also determine the way in which space and time must be united in order to form an abstract medium which shall take the place of their individual selves and which shall be such that the course of Nature in it shall be observable consistently by everyone. The achievement of Einstein is to describe the natural behaviour of bodies in space-time, and he has done so without requiring an artificial division of motion into a uniform and an accelerated part and without the assumption of any forces. His space-time is not homogeneous—it varies from one point to another —and the motion of bodies is such as to satisfy a very simple law. The laws of motion and gravitation thus become united; motions are described simply as they are, and in place of the three abstractions, space, time, and force, we have one, namely, space-time.

It might not be superfluous to linger for a moment on the distinction between absolute and relative space and time, in order to remove a common illusion. It is widely imagined that absolute space and time are facts of observation, and that the space-time of relativity

is something hidden in Nature, the discovery of which affords one more example that things are not what they seem. The truth is, however, that absolute space and time, like space-time, are simply abstractions from observation. They are none of them "facts," but space-time is preferable to absolute space and time separately because it allows the correlation of a range of facts with which the independent conceptions are powerless to deal. The abstract character of absolute space and time is easily recognisable apart from relativity altogether. Consider time, for example. When we say that two intervals of time —say, two successive days—are equal, we are making a completely arbitrary statement which we have no conceivable way of testing. The basis of our time-system is the *postulate* that the Earth rotates uniformly on its axis. There is no meaning in asking whether it *really* does so unless we adopt some more fundamental standard of time-reckoning which must be equally arbitrary. Hence there can be nothing absolute about any time system that we can in practice adopt. The assumption of the Newtonian philosophy is simply that, although it is unattainable, there is an absolute stream of time to which our practical time system approximates very closely. The principle of relativity, on the other hand, says that since absolute time is unattainable it is meaningless to suppose that there is such a thing. Both theories agree that absolute time is inaccessible to actual measurement and therefore is in no sense a fact of observation. The relativity view is preferred, as we have said, simply because it allows a rational correlation of observations which, on the Newtonian view, would be contradictory.

Thus we reach the greatest achievement of the method of abstraction that Science has yet seen. Not only gravitation, but the forces of electro-magnetism also can be dispensed with and the facts which called them into being described in terms of the properties of space-time. The law of conservation of energy also becomes a part of the scheme, and mass is identifiable with energy, so that the whole of the correlations which at the end of the nineteenth century had been brought about by the method of abstraction, with the single exception of the second law of thermodynamics, are merged into one whole.

This wholly inadequate summary can, of course, give no clear impression of relativity considered as a physical theory, but it may serve to make possible an answer to the question: In what way have our ideas of the nature and scope of physical Science been modified by the principle of relativity? Considered in the very broadest way,

the answer is: Not at all. Einstein has simply extended the sway of Newtonian principles over a region of phenomena which Newton never knew. He has started from the facts of observation and submitted his ideas to the test of further observations, by which they have been supported. He has used the Newtonian method of abstracting certain conceptions from the observations, and describing the observations themselves mathematically in terms of those conceptions. By the method of pure abstraction, therefore, he has correlated a much larger body of our common experience than was possible by the Newtonian mechanics, and has augmented our experiences also by those acquired in testing the validity of his scheme. In the broad sense, therefore, so far from having displaced Newton, Einstein has established Newton's principles over a much wider field than they covered before. There is nothing *ultimately* revolutionary in his work. It is just as much, and just as little, metaphysical as was Newton's; it is related to experience in just the same way as was Newton's. The only difference is that it is more comprehensive.

In becoming so, however, it has had to assume a completely new appearance. It uses abstractions of the same kind as Newton's, but they are entirely different abstractions. They are fewer and simpler, although, naturally, less familiar. Yet it is perhaps not impossible that they are more closely allied than we imagine to what our instincts might have told us of Nature if we had not inherited the Newtonian tradition. Newton took the trouble to explain time, space, place, and motion because, as he says, "the vulgar conceive those quantities under no other notions but from the relation they bear to sensible objects"—which is precisely the relativity conception of them. To Aristotle, again, space was not an infinite void, but "the boundary of the enclosing body on the side of the enclosed." Further, Einstein's abolition of the division of motion into a uniform and an accelerated part is essentially a rehabilitation of the Aristotelian maxim that "of one simple body, one sole simple motion can be natural," which Simplicius, in Galileo's *Dialogues*, urged against Copernicus. The all-important difference, however, between Einstein and Aristotle is that Aristotle was laying down an arbitrary principle, whereas Einstein is describing observations.

The net result of Einstein's great theory, then, is that we can now regard the whole mechanical and electro-magnetic phenomena of Nature as a manifestation of the characteristics of one abstract medium—space-time, or ether, or whatever else we care to call it.

The Newtonian conceptions of space, time, mass, gravitation, momentum, as well as energy and electric and magnetic forces, all take their places as specified peculiarities of this medium. To Newton, space and time were the stage on which the drama of forces and motions was played. To Einstein, the drama is merged into the stage; the play is the scenery. Abstraction can hardly go further. It has made the diverse phenomena of Nature into a universe, but at the expense of all individuality. The world is united, but featureless. Other methods must be employed if a more intimate knowledge of the relations between phenomena is to be obtained, and the method pre-eminently suitable for the purpose is that of hypothesis. We turn, therefore, to the second great development of this century, that of the atomic hypothesis, which is embodied in the quantum theory.

CHAPTER V

THE EXTREMITY OF HYPOTHESIS—THE QUANTUM THEORY

THE details of the quantum theory, like those of relativity, are remote from our purpose; again we want only the essential character of the new development. We left the atom, it will be remembered, closely veiled from observation, but nevertheless somewhat arbitrarily accredited with Newtonian mass and extension and made the sport of Newtonian forces: a "billiard ball" was the orthodox symbol. At the very end of the nineteenth century, however, the utility of the atom for billiards was found to be quite illusory. In spite of its name it was broken to fragments, and the necessity arose of assigning to the parts the additional concept of electric charge. This was not a concept formed by Newton, but it had the authentic Newtonian character, and its bestowal on the parts of the atom permitted the correlation of a wider field of experience than that amenable to the billiard ball. A definite step forward had been taken by the use of Newtonian principles, albeit they were applied to hypotheses, which Newton abhorred.

The successor of the billiard ball eventually took the form of the solar system—an atom with a central Sun of positive electricity (an agglomeration of positive *protons* and negative *electrons*) surrounded by revolving planets which were simply electrons. Physicists were fond of billiards, however, and instead of giving up the conception of a billiard ball altogether they tacitly transferred it from the whole atom to the constituent protons and electrons. Furnished, therefore, with an atom whose *parts* could be manipulated, and having the additional conception of electric charges as centres of electric force to play with, they were very favourably situated for the incorporation of previously refractory experiences into the atomic scheme. In their rashness they attempted to incorporate the phenomena of the emission and absorption of light, and the fat was in the fire.

The old billiard-ball conception had contrived to reach a venerable age through its humility: it made no attempt to deal with phenomena beyond its powers, and its absolute quiescence in face of them gave no indication at all of what kind of substitute would be likely to succeed where it was impotent. It did its humble work well, and so

was retained in office. But the solar-system conception, young and eager, rushed in where the billiard ball feared to roll, and in so doing brought about its own destruction. It is here that we come to the significant part of the modern quantum theory.

The solar-system model of the atom showed a *prima facie* capacity for emitting and absorbing light (or, more generally, radiation) because its parts were electro-magnetic in character and radiation had already been interpreted as electro-magnetic wave motion. But a serious difficulty arose. A process of emission of radiation could be conceived readily enough, but by the established laws of electro-magnetism it inevitably required the almost immediate destruction of the atom by the mutual cancellation of its protons and electrons. A dilemma was therefore created. Either the solar-system model of the atom, to which the development of the atomic hypothesis had apparently inevitably led, or the laws of electro-magnetism, abstracted from phenomena, had to be left out of consideration if the atomic hypothesis was to be extended to include the interaction of matter with radiation. And physicists in general were willing to abandon neither.

Let us pause for a moment to look at the situation. We can see it clearly enough now, and the position does not appear nearly so desperate as it did at the time. As we have already said, atoms could be endowed with any properties so long as their consequent behaviour in bulk agreed with observation; they were under no obligation to accept characteristics which belonged to phenomena, and therefore there was no compulsion at all to subject them to the laws of electro-magnetism. Why, then, did physicists regard their insubordination to those laws with such concern?

It was simply that they did not realise the possibilities of hypotheses. The atoms, being hypothetical units, were in their hands to mould to the dictates of their imaginations, and they did not know their own freedom. They thought of atoms, not merely as hypotheses but as potential phenomena and therefore necessarily subject to the laws already established for phenomena by the method of abstraction. The process began, as we have seen, when atoms were first introduced into physics. At that time they were supplied instinctively with mass and the other Newtonian abstracted qualities, and the implication that they were potential phenomena took such hold of succeeding physicists that by the twentieth century its arbitrariness needed a genius to perceive it. Fortunately, the genius,

in the person of Niels Bohr, was at hand. Bohr retained the solar-system model of the atom, but absolved it from obedience to the laws of electro-magnetism.

This step of Bohr's was the most significant in physical science since the introduction of the hypothesis of atoms. What virtually it did was to establish the fact that the hypothetical atoms were pure conceptions, that they belonged essentially to a different category from the facts of observation. They were creatures of the imagination, to be formed into the image of our fancies and restricted by whatever laws we cared to prescribe, provided only that when they behaved in accordance with those laws they should produce phenomena. They were removed from the realm of experience and deposited in that of reason. This is the essence of the famous "quantum theory," though it is not the aspect under which it was first revealed and from which it derives its name.

Developments have succeeded one another with almost alarming rapidity, but from our point of view nothing fundamentally new has happened. The solar-system model, as we have hinted, has gone and a conception devoid of any pictorial aspect has taken its place, but that—if in so speaking we may disclaim any disrespect to the brilliant physicists who have organised the process—is but the ass's kick at the dead lion. Whatever formal doctrine physicists may profess, they exhibit in practice no more belief in the phenomenal reality of atoms than in the philosopher's stone. We might well leave the subject here, were it not that the more recent developments have been assigned a significance outside the realm of physics which it is important that we should consider. As briefly as possible, therefore, we will try to indicate what those developments are.

The electrons and protons, of which atoms are regarded as composed, are no longer thought of as billiard balls, although they retain some of the properties of those objects. Other properties which it is necessary to give them resemble those of waves, and if we wish to form mental pictures of them, we must unite in our minds the images of billiard balls and those of waves. But this is impossible—partly, for instance, because a billiard ball is sharply bounded and a wave is not. Hence we can form no such mental pictures. The natural meaning of this, in the light of our previous considerations, is that the conceptions of space and time (or space-time) abstracted from phenomena cannot be applied to them.

This is no great matter, however, provided that our conception of

them is definite: if they cannot enter experience they may still remain rational. So far they have done so. We can represent them by symbols to which we can ascribe mathematical properties, though not pictorial ones. By virtue of these mathematical properties their behaviour in various sets of circumstances can be worked out and a steadily growing volume of observations thereby correlated. At each absorption of a new fact into the scheme of correlation a modification in some degree of the conception of the atom may be required, and the whole course of development of the conception is determined by the necessity of correlating observations. Well might we feel, like Galileo, that we cannot find any bounds for our admiration how that reason is "able to commit such a rape upon the Sences as in despight thereof to make herself mistress of our credulity."

The old atom was a "substance" and its behaviour a series of "actions": the new atom is a set of concepts and its behaviour can be indicated only by propositions or principles. One of these propositions, which has been introduced by Heisenberg as a fundamental one, is known as the "principle of indeterminacy" or "principle of uncertainty." I will use the latter term, as I want to speak of indeterminacy in a more general sense later. This principle gives something approaching a definite limit to the extent to which the ordinary concepts of "position" and "momentum" can be applied to ultimate particles such as the electron. An electron can be said quite definitely to have a position in space if we do not attempt to consider it as having momentum, and it can be said quite definitely to have a momentum, measured in the ordinary way in terms of mass, space, and time, if we do not attempt to consider it as having a position in space at any instant. If, however, we try to apply to it simultaneously the conceptions of momentum and instantaneous position in space, we are attempting the impossible; the electron is such that in relation to it the two concepts are mutually contradictory.

The principle of uncertainty states more than this, however. It states that if you try to force these concepts on the electron, you can do so if you are satisfied with a certain amount of vagueness. Thus, if, instead of stating the momentum with the absolute accuracy which is permissible when position is not considered, you are content to say that the momentum lies between certain limits, then the concept of position, pleased at the concession, condescends also to become applicable within certain limits; and the greater the degree of indefiniteness you are willing to allow in the specification of the

momentum, the greater will be the precision with which the position can be specified. You can, therefore, state both the position and the momentum of an electron if you say only that each of them lies between certain limits; and, further, the product of the ranges of uncertainty of the two concepts is equal to or greater than a definite quantity—known as "Planck's constant," and represented by h.

It will repay us to spend a little time considering this principle, for it is not only an excellent illustration of the methods of modern physical thought, but also a result which is likely to direct the course of theoretical physics in the immediate future. It has been arrived at, of course, by its power of co-ordinating observations, but it claims its status of fundamentality by the application of the principle, familiar to students of relativity, that anything which is apparently of an observable character but yet is consistently concealed from us by Nature, must not be employed in any capacity for the purposes of physics. Such things are often called "unobservables," but because of certain qualifications which I will state immediately, I will refer to them as "inaccessibles." The qualifications are these: In the first place, the concealment must not be merely the result of a practical difficulty; it must be due to a kind of conspiracy between natural phenomena. Thus, the interior of the earth cannot be observed, but we need not on that account leave it out of consideration in discussing the propagation of earthquake shocks. Secondly, the concealment must be not only from direct observation, but also from deduction. An electron cannot be directly observed, but we employ it in physics. Its mass, for example, is deducible from certain experiments, although the electron cannot be directly weighed, and we therefore retain the conception of the mass of an electron. The absolute velocity of a body, on the other hand, although it appears perfectly susceptible of measurement, always eludes us because of a working together of natural laws to that end. Every experiment made to determine it has failed, not because of imperfect apparatus but because the effect looked for has always been automatically and exactly compensated for by an equal and opposite one. This is interpreted as an indication that such compensation is inevitable, that it arises from the nature of things, and so we dismiss absolute velocity from the scheme of physics and substitute the principle of relativity.

Now when we try to imagine an experiment which will give us simultaneously the momentum and position of an electron, we find

we cannot do so. To take a partial example for simplicity: you can conceivably observe an election only when it sends you light, and to send you light it must of necessity move. You can, therefore, observe its position only at the expense of a change of momentum, and therefore the initial momentum is inaccessible. If the electron does not move you cannot see it; if it does move it has lost the momentum you set out to observe. Hence the inaccessibility of the momentum arises not from practical difficulties but from the nature of observation itself. It is, therefore, regarded as an indication that the simultaneous specification of position and momentum of an electron is an illusory ideal: we must rule out of physics everything that presupposes it to have a meaning.

In so doing we must be ruthless. It is not sufficient to say that the electron *has* a simultaneous position and momentum but that we cannot find them out. We must say there is *no meaning* in the concepts when applied to electrons. The principle of uncertainty does not limit the accuracy of our measurements; it helps to define an electron. The "uncertainty" of our determination is not a human failing; it is a measure of some property of the electron which can properly be defined only in terms other than those appropriate to sensible matter. We approach a realm of pure conception with minds prepossessed by the abstractions of phenomenal physics. The straightforward course would be to clear our minds entirely of those preconceptions before entering the city gates, but that is a counsel of perfection. As human beings, the most that can be expected of us is that we shall be ready to shed them at the first indication that they are illegal and to substitute whatever conceptions the laws of the realm prescribe. The principle of uncertainty is a warning that the space and time abstractions are prohibited; it does not yet appear what should take their place.

Let us pause here a moment and try to realise what this means in terms of ordinary thought. If the reader is trying to picture in his mind what kind of thing that can be which does not occupy space or persist in time, he should at once stop; he is on the wrong track. That is something which no one can do; Heisenberg himself cannot do it. If the reader concludes from this that he cannot understand the trend of modern physics and may as well give up the attempt, he is again on the wrong track, because the trend of modern physics is not at all in the direction of forming mental pictures. If this is clearly recognised and kept in mind, I do not think the general

course of the new developments presents any insuperable difficulty to the ordinary non-mathematical thinker.

Let us look at the matter in this way. Suppose a man, fully intelligent but with no knowledge of ordinary life at all, is suddenly placed on the Earth and set to make a study of clouds. He sees that a cloud is massive, opaque, white or grey in colour, has movement, changes shape, and so on. He is told, however, that certain small spots on the ground are connected with the cloud, and he is asked to find the connection. He ultimately arrives at the conception that the cloud is not continuous, as it appears to be, but is composed of myriads of small spherical droplets of clear transparent liquid. He has never experienced such things, and so cannot imagine what they are like, but he can assign to them properties—such as weight, volatility, reflecting and refracting powers with respect to light, and so on—which are quite at variance with his experience of the cloud, but which, nevertheless, when applied simultaneously to the members of an innumerable crowd of droplets, result in just the characteristics observed in the cloud. His companions cannot at first understand what he is doing. They try to imagine these droplets, but they can do so no better than he can: their experience is of an opaque something floating in the air, and he speaks of transparent bodies falling to the ground. But if they cease to plague their imaginations and, regarding the unfamiliar words simply as symbols, calculate the behaviour of the multitude of droplets, they will find that, for example, opacity comes out in the end although each particle was assumed to be transparent. Substitute time and space for opacity and the unknown characteristics of atoms for transparency, and you have an analogy of the present position in physics. The ordinary person has no need to bother about the mathematical details, but I see no great difficulty in his understanding that unimaginable concepts, when properly defined and combined together according to definite rules, may result in collective properties corresponding to our ordinary experience.

It should be added that this, like all illustrations, is imperfect. Its chief defect is that, whereas the hypothesis of droplets could be verified by observing a shower of rain, electrons are inherently unobservable.

So far I have described the principle of uncertainty more or less in the customary way. I want to call attention now to one or two points in which the ordinary statements of the principle seem to

me to be open to criticism. The rule of excluding inaccessibles from physics is often spoken of as if it were a fundamental philosophical principle: I think it is really only a rule of convenience. It supposes a kind of omniscience in us, by which the physical world is necessarily composed only of that which, with our present knowledge and means of observation, we can conceivably detect. If, however, an objector should say that there might be more things in Nature than are dreamed of in our philosophy, we can only reply that until we dream of them we shall assume, and act on the assumption, that they do not exist. This is perfectly justifiable so long as we realise that the objector also might be right.

The essentially pragmatic character of the rule is seen in a particularly interesting way in the principle of uncertainty. We speak of observing an electron. But an electron is in any case merely a concept, and the idea of observing a concept, being itself symbolical, can impart nothing but a still more subtle symbolism to a grave discussion of the mechanism of observation. For an electron is congenitally not a possible object of observation; it is a part of an ideal scheme of concepts which are designed to correlate observations of material bodies. What we really mean by saying that we cannot observe its position when its momentum is given is that the assumption that it is a picturable object, having a definite position, introduces confusion into the scheme of correlation.

The symbolical character of the principle is particularly heavily obscured in the statement, which is sometimes made, that you disturb an electron by looking at it. Even apart from the already mentioned symbolism of the expression, "looking at an electron," the statement is misleading, for it is not even this symbolic process that disturbs the electron, but the physical conditions which make the process possible. It is not *looking* at an electron that disturbs it, but *illuminating* it, whether you then look at it or not. The act of observation is subjective, and the statement in question suggests a degree of interaction of subjective and objective processes which is not inherent in the principle of uncertainty at all. The electron would still be moved out of its position in the atom in the act of emitting light, whether that light entered a person's eye or not; observation in the strict sense has nothing to do with the matter.

It might perhaps be suggested that, since this is so, the principle of uncertainty expresses merely a property of light, and that if in

the course of time the human race develops a means of observation through another agency, the principle will cease to be true. If the electron were actually a proper object of observation, existing by its own right in an external world, I think this suggestion would be valid. We should still be justified in adopting the principle for convenience in the present state of physics, but we should have to give up the idea that it expresses anything fundamental about electrons. But actually the electron has been called into existence to correlate the observations that we have been able to make with our present faculties. The forms which it assumes and the laws under which it acts are shaped by the necessity of unifying those observations and no others. If we acquired a new sense, making possible experiences of a new type in addition to those we already possess, no doubt the properties assigned to electrons would be profoundly modified, and as likely as not the conception of an electron would have to be abandoned altogether. The electron is essentially a creature of the world as we know it, and therefore any statement we are led to make about electrons is not subject to adaptation to the conditions of a world we do not know.

Let us try to crystallise the salient points of this very intricate matter into a few sentences. The hypothetical atoms and their parts, which were at first instinctively subjected to the abstractions derived from the study of observed bodies, were liberated by Bohr and made recognisable as concepts capable of assuming any form and willing to serve under the sway of any principles which might be found necessary to enable them to correlate observations. Accordingly they have been released from the duty of presenting a spatio-temporal aspect, and in fundamental researches no attempt is made to conceive them in a pictorial form. The laws which shall govern them have not been laid down, but some hint of the nature of those laws is given by the principle of uncertainty. This principle connects in a specified way the degrees of inapplicability to electrons of the concepts of momentum and position in space. It has been given fundamental importance by the principle that any notion which the nature of things as we know them does not allow us to arrive at by observation or deduction from observation, shall be removed from consideration in physics. As it relates to electrons, this principle simply says, in other words, that the assumption that electrons have simultaneously a definite position and momentum leads to contra-

dictions in the scheme of concepts by which observations are correlated.

It will be seen from this that, as we have already remarked, the significant step, from the point of view of the nature of Science, was the liberation of the atomic hypothesis* from the abstractions of phenomena. The subsequent developments, though of profound physical importance, introduce no new element. We must now regard the whole atomic scheme as purely conceptual, and take care that we are not again enslaved by the idea that its elements are potentially observable. There are some who believe that the present non-picturable atom is only a temporary element of thought, and that the time will come when we shall again visualise an atomic model. It may be so, but the point is unimportant. The significant thing is that we are free to make the atom whatever we like for the purpose of correlating observations. If we can make it in the image of a machine, well and good; if not, well and good also. The form the atom assumes is a matter of detail; the liberty to conceive of it as we please is what matters.

Before we proceed to discuss the general significance of these new advances in physics we might profitably take a final comprehensive glance at the relativity and quantum theories in order to sum up the character of their so-called "unintelligibility." Relativity is an abstraction, and the quantum theory is a hypothesis. The apparent absurdity of relativity is entirely a matter of limited experience, and that of the quantum theory is entirely a matter of limited thought. Relativity teaches us that if we were to travel about the universe at high speed we would discover, on returning to the Earth, that a longer time had elapsed than our perfectly accurate clocks would indicate. We find that difficult to believe, but that is only because we have never travelled in such a way; if we could do so we should find the predicted result, and after a few repetitions we should cease to think it strange. We can say that with confidence because we have obtained the equivalent experience in physical experiments, although the moving bodies involved are not human beings. But the fantastic character of the quantum theory is quite different. No possible

* I use "atomic hypothesis" as a general term for the whole of the modern "microscopic" physical theory. Strictly speaking, of course, the theory includes protons and electrons individually, as well as when they are associated into atoms, but the details of the scheme are so interdependent that they are best regarded as a whole, under a single name.

extension of experience can make us familiar with an electron which does not occupy time and space,* for the electron is not something imaginable, but a pure thought-structure. It is therefore useless, and even positively harmful, to try to picture it. A more vivid imagination would help us to resolve the paradoxes of relativity, but it would simply hinder us in trying to resolve those of the quantum theory: what we need there is greater power of logical thought. Relativity teaches us about phenomena because the notions it deals with are abstracted from our experience of phenomena; it surprises us because it reveals possibilities of experience which we never contemplated. The quantum theory surprises us because it reveals *impossibilities* of experience which we never contemplated; we thought we were justified in extending to atoms the abstractions discussed in relativity, and we find that we were not. The combined effect of the two theories therefore, is to give us truer notions of the limitations of experience. It still remains for us to explore the possibilities of thought.

* Or space-time. It must be clearly understood that the supersession of space and time in atomic physics is totally distinct from their supersession in relativity. In relativity space and time are still employed as partial and variable aspects of the one absolute abstraction, space-time, which alone permits of the correlation of observations of persons in relative motion. But in atomic physics neither space and time separately nor space-time has any significance. The appropriate substitutes are not yet known.

CHAPTER VI

THE COMMON-SENSE OF IT ALL—EXPERIENCE AND PHYSICAL THEORY

WE have now traced the course of physical Science up to the present moment. We have encountered a vast body of data comprising observations potentially common to all normal people, and we have seen that these observations have been correlated in a very large measure by the two processes of abstraction and hypothesis. Abstraction has led us virtually to a contorted space-time, and hypothesis to a scheme of concepts unpicturable by the imagination. Both space-time and the scheme of concepts, however, by obeying prescribed rules, reproduce the data of observation, so that out of pure conceptions, having only a rational meaning, we can evolve, as it were, a very large part of the world of experience. This is the great achievement of modern physical Science. The question that next arises is: What is the relation, in the category of reality, however we may define that word, of the world of experience to the connecting world of thought? This question can be most conveniently approached by way of the conclusions reached by Sir Arthur Eddington and Sir James Jeans.

The problem has been admirably framed by Eddington: let me quote what he says.* "I have settled down to the task of writing . . . and have drawn up my chairs to my two tables. Two tables! Yes; there are duplicates of every object about me—two tables. . . . One of them has been familiar to me from earliest years. It is a commonplace object of that environment which I call the world. How shall I describe it? It has extension; it is comparatively permanent; it is coloured; above all it is *substantial*. . . . Table No. 2 is my scientific table. . . . There is nothing *substantial* about my second table. It is nearly all empty space . . . pervaded by fields of force." In other words, there is the table of experience and the table which is a mathematical function of the unimaginable concepts we have just referred to. What is the relation between them?

Eddington's answer is definite: the conceptual world is symbolic of the world of experience. "Science," he writes,† "aims at con-

* *The Nature of the Physical World*, p. xi.

† *Ibid.*, p. xv.

structing a world which shall be symbolic of the world of commonplace experience. It is not at all necessary that every individual symbol that is used should represent something in common experience or even something explicable in terms of common experience. The man in the street is always making this demand for concrete explanation of the things referred to in science; but of necessity he must be disappointed. It is like our experience in learning to read. That which is written in a book is symbolic of a story in real life. The whole intention of the book is that ultimately a reader will identify some symbol, say BREAD, with one of the conceptions of familiar life. But it is mischievous to attempt such identifications prematurely, before the letters are strung into words and the words into sentences. The symbol *A* is not the counterpart of anything in familiar life. To the child the letter *A* would seem horribly abstract; so we give him a familiar conception along with it. '*A* was an Archer who shot at a frog.' This tides over his immediate difficulty; but he cannot make serious progress with word-building so long as Archers, Butchers, Captains, dance round the letters. The letters are abstract, and sooner or later he has to realise it. In physics we have outgrown archer and apple-pie definitions of the fundamental symbols. To a request to explain what an electron really is supposed to be we can only answer, 'It is part of the A B C of physics.' "

This is clear enough, but is the matter to end there? If so, Science is merely a form of Art, and an absurdly intricate one, too. Why should we laboriously seek to express the obvious in terms of the incomprehensible, when any poetaster can give an intelligible symbol of the world with infinitely greater facility? Apart from practical considerations, there are, so far as I can see, only two possibilities which can justify such a procedure: first, that the conceptual scheme is in some sense "truer" than the world of experience; second, that it reveals the existence of a connecting link between the diverse elements of experience. The fundamental point at which I diverge from Eddington and Jeans is that they favour the first possibility and I favour the second.

I have previously (p. 44) quoted a passage in which Eddington expresses a belief, which he evidently shares, in the "real truth" of theoretical physics. Jeans is even more emphatic. "If we want to understand the fundamental nature of things," he writes,* "it is to

* *The Mysterious Universe*, p. 44.

these small scale phenomena* that we must turn our attention. Here the ultimate nature of things lies hidden." No doubt both of these writers would accept also the second justification of the physical scheme—that it correlates experiences—but this aspect of the matter has such little prominence in their minds that they scarcely refer to it. It is pointedly ignored, in fact, in Eddington's analogy between electrons and the alphabet. The fact that the letters S T A R form the word "star" and also the word "rats" implies no connection at all between the objects represented by those words, whereas the existence of electrons in the star and in the laboratory is of the utmost significance in the interpretation of stellar conditions. It is clear that the real significance of the physical scheme to both Jeans and Eddington is that in some way it is a truer expression of that in Nature which we may regard as external to us than is the world of ordinary experience.

I do not know how any direct decisive test can be made between these two valuations of the physical scheme; so much depends on the answer we give to Pilate's question. The conclusions to be drawn from them, however, differ sharply, and some of these will be discussed presently. In the meantime I would suggest one or two considerations which seem to me to carry much weight. In the first place, if we regard as the radical element of Science that which is continuously progressive, and as the incidental elements those which are constantly changing character, then there is no doubt that the essence of Science is expressed by the definition given in the Introduction—that is, in brief, by discovery and correlation, for these are the only elements whose progress is continuous. Theories and hypotheses change almost from day to day, and if we are to regard the "small scale phenomena" as truth, then truth in Science must be either exceedingly protean in form or else an ultimate ideal which can be reached only when the structure of Science is complete. I do not think either alternative would entitle Science to be taken seriously by philosophers or theologians in their quest after Truth. This point is exemplified very strikingly in Jeans's recent book, *The Mysterious Universe*. After likening the course of modern physics to that of a winding river, he draws conclusions about the "designer of the universe" in sentences which, taken alone, have a dogmatic aspect, and qualifies them at the end by saying (p. 150) that "perhaps

* By "small scale phenomena," of course, is meant what we are calling the atomic hypothesis.

. . . science should leave off making pronouncements: the river of knowledge has too often turned back on itself." It would seem to be simpler and less misleading not to draw the conclusions.

Another objection to the view that the conceptual world of physics is the "truth" about Nature is that it makes Nature a much more comprehensive thing than our experiences give sanction for, and leaves arbitrary the principle on which the selection of concepts capable of being experienced is made. We construct a conceptual scheme in order to correlate experiences. The scheme, when constructed and set working, is of course capable of logically grinding out those experiences, but it grinds out many other things also, apparently identical in character, which happen not to be actual—pure mental structures which correspond to nothing we experience. The view that the conceptual scheme is the truth about experience implies that these structures are just as much a part of Nature as are the others. "The things which we might have built but did not," says Eddington,* "are there just as much as those we did build." The reason why we did not build them can only be suggested in terms of evolution; probably the developing mind had no use for them, and so lost the power, if it ever had any, of apprehending them. No criterion, however, for distinguishing useful from useless structures can be imagined. A somewhat similar case is provided by the possibility of the backward running of time which many of the laws of physics include, but that raises other questions which are irrelevant here.

This procedure of endowing Nature with an indefinite amount of "lumber," as Eddington calls it, is very reminiscent of the Red Knight's plan, of which Eddington himself has elsewhere made such a timely application:

For I was thinking of a plan
  To dye my whiskers green,
And then to use so large a fan
  That they could not be seen.

There seems little justification for supposing that a scheme devised to symbolise observations can tell us of entities coequal with those observed, but somehow unobservable. If, however, we regard the scheme simply as an instrument for correlating observations, this additional capacity which it possesses is of no significance. It does

* *The Nature of the Physical World*, p. 241.

credit to our imaginations, whose reach exceeds the grasp of the senses, but it tells us nothing about the physical world. The word "foreigner," used to describe visitors to England who are not of British nationality, would also be a true description of Martians if there were any, but it is no less satisfactory on that account, nor can we regard its employment as establishing the habitability of Mars.

For these reasons, then, as well as for the reason that an unverifiable hypothesis can be, at best, only one of an indefinite number of possibilities, I think that we must deny to the conceptual scheme of physics any capacity for telling us the truth about Nature beyond what is implied by its power of correlation. What it does show is that a unity exists in a field of diverse phenomena. So far as it extends it establishes the fact that we are capable of making a rational coordination of our experiences. Whether the unity we find is only in our experiences, and is therefore a quality of our minds, or whether it belongs to an external Nature, is, as I have previously said, a question beyond our scope. But in either case, the unity is what we can definitely assert, and its extension to an ever-growing field of phenomena is the continuously progressive element in physical Science. The scheme of correlation by which it is established is perhaps but one of many possible schemes, and is certainly an ever-changing thing. It does not seem reasonable to choose the evanescent rather than the persistent as the characteristic mark of Science.

This question of the rank to be assigned to the physical scheme lies at the root of a number of important problems on which, as it seems to me, Eddington and Jeans have reached invalid conclusions. The two most important of these relate to the place of measurement in Science and physical determinism. I will discuss them in turn.

CHAPTER VII

SCIENCE AND MEASUREMENT

EDDINGTON'S view of the place of measurement in Science is expressed in the following sentence:* "the cleavage between the scientific and the extra-scientific domain of experience is, I believe, not a cleavage between the concrete and the transcendental but between the metrical and the non-metrical." In a previous discussion of this matter he seemed to identify only the domain of the "exact" sciences with the metrical elements of experience, but here he removes this restriction and speaks simply of Science.† According to Eddington, therefore, nothing that is not metrical in character can be treated scientifically. Jeans goes still further in the same direction. To him, not only Science, but the whole external universe, is metrical. "The final truth about a phenomenon," he writes,‡ "resides in the mathematical description of it; so long as there is no imperfection in this our knowledge of the phenomenon is complete." This conclusion, which is practically identical with that reached long ago by Kepler and others, seems to me to be contrary not only to reason but to actual fact.

The question is of the highest importance, because the authority of Jeans and Eddington has been widely invoked by non-scientific thinkers intent on "putting Science in its place." It is usually a very simple matter to decide whether an experience is metrical or non-metrical in character, and a ready solution of many of the difficult questions raised by Science is available if we can simply ignore

* *The Nature of the Physical World*, p. 275.

† Sir Arthur Eddington has told me in conversation that in this connection he uses "Science" and "exact Science" as synonymous terms. The definition of Science given in the Introduction to this book would therefore presumably be much too general for him to accept. The point at issue might, I think, be fairly expressed in this way: to Eddington the rift between the physical and biological sciences is more fundamental than that between biology and, say, theology, so that the word "Science" is properly applicable only to generalised physics; my view, on the other hand, is that the physical and biological sciences are much more akin to one another than either of them is to theology, and by contrast are entitled to a single comprehensive name, such as "Science."

‡ *The Mysterious Universe*, pp. 140–141.

everything that Science has attempted to say of non-metrical experiences. Artists, theologians, metaphysicians, and moralists are thus enticed into what I believe to be a fool's paradise. Not only so, but this false escape from the challenge of Science is necessarily accompanied by a real deprivation of its benefits. Art and religion have much to gain by a proper use of scientific principles, and the sharp restriction of the domain of Science to the metrical elements of experience leaves them the poorer.

It is of course obvious that a large part of the data of Science is non-metrical in character. The schoolboy's name for chemistry is "stinks," not "balances," and a very appropriate name it is. Biologists observe the flight of birds very closely, but they do not trouble to apply the Fitzgerald–Lorentz contraction, not because it is too small to be important, but because it has no relation to the *kind* of observation they are interested in. It is clear, therefore, that much of the recording and augmentation of our experiences, which is essentially scientific, is not metrical. This is in itself sufficient to refute the doctrine in question: we need look no further in order to disillusion the non-scientific thinkers to whom I have referred.

But this is not the whole of the matter. I have no doubt that Jeans and Eddington would admit this readily enough, and still adhere to their opinions. For to them observations are just convenient tools for leading us to the truth underlying phenomena: it is that truth which they claim is metrical. Their doctrine applies not to the collecting of experiences, but to their rational correlation, and they would say that when we come to analyse our experiences in order to discover the microscopic scheme of Nature, it is only the metrical elements that we can employ scientifically. I observe a cup, for example, and I notice that it is yellow in colour and hard to the touch. Those experiences I share with other normal people, and they are not primarily metrical. But when they are absorbed into the scientific scheme, it is only the metrical part of them which is used. The yellow colour, which I happen to dislike and of which someone else might be very fond, is represented only by a range of "wave-lengths," about which neither of us has any emotions at all. The hardness is represented by "electro-magnetic forces" (or characteristics of "space-time") which are definable by means of equations. Through these metrical quantities, all that is scientifically tractable in the yellowness and hardness of the cup is expressed, and the other qualities of yellowness and hardness are left over as belonging, according to

Eddington, to the extra-scientific domain of experience, or, according to Jeans, so far as I can gather, to the domain of illusions.

Now this attitude represents a return to the position of Galileo, and, from one point of view, a very welcome return. We have seen that, to Galileo, all qualities (such as colour and heat, for example) which were not mathematical were subjective. In the later developments of physics, however, it was found that many of these phenomena were amenable to scientific treatment, and the remarkable success of the Newtonian abstractions and the hypothetical scheme of atomic physics led to an unwarranted belief that the whole of experience would ultimately be explicable in terms of physical quantities. Thus arose the old type of materialism which stigmatised everything not so explicable as fantasy. But the deeper knowledge of recent years has shown that the existing developments of physics can deal only with experiences which can be represented by numbers, and that although many more of our experiences than Galileo thought are so representable, they are far from being the whole. It follows that the old materialism has lost its foundation. Strictly speaking, it never had any foundation but only a sort of superficial probability; the disappearance of even that, however, is matter for satisfaction.

But to deduce from this that the non-metrical elements of experience are outside any kind of scientific treatment seems to be a repetition of the old blunder in the opposite direction. It was unjustifiable to suppose that all experience could be treated by the present physical methods: it is surely equally unjustifiable to suppose that the present physical methods include the whole of Science. This, however, is necessarily involved in the idea that Science can treat only of the metrical. Such an idea requires that Science is a closed, self-contained system, including all that is metrical in our experience. This Eddington very clearly points out. But it is difficult to see how the existence of this closed system can be established. Even in the metrical part of our experiences there are phenomena which lie outside it. Take motion, for example. The system includes the motion of a comet, but it does not include the motion of a fly. We need consider none of the non-metrical aspects of the fly, but only its motion as a piece of matter. The matter is made up of protons and electrons, formed into atoms indistinguishable from those of the comet, and its motion can be described completely in terms of space and time. Nevertheless, the motion of the fly is essentially of a different character from that of the comet; it cannot be included

within the closed system of metrical physics. Although itself metrical, we can make nothing intelligible out of it unless we associate it with something non-metrical, which we call "life." And if anyone thinks that motions associated with life are so entirely incalculable as to be outside Science, he should reflect for a moment on the significance of a fly-paper.

The fact is that Science is fitted to deal with all experiences which are common to all normal people. Such of these experiences as are metrical in character are largely—but, as we have just seen, not entirely—susceptible to correlation by the present scheme of physics. The others appear to be amenable only to conceptions which are individually different, but ultimately of the same character. For these experiences also we employ abstractions and hypotheses. The abstraction of space-time is irrelevant, so we leave it in the phenomena and instead take out such concepts as life, mind, will. These are just as truly abstractions as are space and time; the "I" of psychology is as valid a scientific idea as the "*i*" of mathematics, and has perhaps still more right to be called an imaginary quantity, for it can at least be imagined. Similarly, we employ hypotheses. The hypotheses of protons and electrons are irrelevant, so we conjure up such ideas as organic evolution and subconsciousness. We observe and we correlate by the same methods as those which we employ in physics, and to a certain extent we can predict events. In every respect our treatment of these experiences has the same character as that of the metrical experiences. It appears to be an arbitrary and extremely inconvenient use of language to call the one treatment scientific and the other not.

Eddington gives an admirable example of the limitation of Science to measurement which it is worth while to quote, because it shows, as it seems to me, both the strength and the weakness of his position. He says:* "If we search the examination papers in physics and natural philosophy for the more intelligible questions we may come across one beginning something like this: 'An elephant slides down a grassy hillside . . .' The experienced candidate knows that he need not pay much attention to this; it is only put in to give an impression of realism. He reads on: 'The mass of the elephant is two tons.' Now we are getting down to business; the elephant fades out of the problem and a mass of two tons takes its place. What exactly is this two tons, the real subject-matter of the problem?

* *The Nature of the Physical World*, pp. 251–252.

It refers to some property or condition which we vaguely describe as 'ponderosity' occurring in a particular region of the external world. But we shall not get much further that way; the nature of the external world is inscrutable, and we shall only plunge into a quagmire of indescribables. Never mind what two tons *refers* to; what *is* it? How has it actually entered in so definite a way into our experience? Two tons *is* the reading of the pointer when the elephant was placed on a weighing-machine. Let us pass on. 'The slope of the hill is 60°.' Now the hillside fades out of the problem and an angle of 60° takes its place. What is 60°? There is no need to struggle with mystical conceptions of direction; 60° *is* the reading of a plumb-line against the divisions of a protractor. Similarly for the other data of the problem. The softly yielding turf on which the elephant slid is replaced by a coefficient of friction, which though perhaps not directly a pointer reading is of kindred nature. No doubt there are more roundabout ways used in practice for determining the weights of elephants and the slopes of hills, but these are justified because it is known that they give the same results as direct pointer readings.

"And so we see that the poetry fades out of the problem, and by the time the serious application of exact science begins we are left with only pointer readings. If then only pointer readings or their equivalents are put into the machine of scientific calculation, how can we grind out anything but pointer readings? But that is just what we do grind out. The question presumably was to find the time of descent of the elephant, and the answer is a pointer reading on the seconds' dial of our watch."

Now the whole secret of the matter is in the last sentence, which is added as a sort of after-thought: "The question presumably was to find the time of descent of the elephant." Naturally, since the time of descent is essentially a metrical quality we should expect the relevant parts of the data to be metrical in character. But suppose the further question is put: "To find the damage done to the elephant." "Two tons" is of no use now; the living, struggling, trumpeting animal must be reckoned with. We can do without the knowledge of the slope of the hill, and the coefficient of friction "leaves us cold." As before, the poetry fades out of the problem, and this time it takes the metrical elements with it; but there is still something left, and that something is scientific in character. It involves such things as abrasions and broken limbs; it is approachable with chloroform and X-rays; the problem requires a knowledge of

the anatomical structure and physiological processes of elephants—that is, scientific knowledge; and the answer can be stated in scientific terms conveying the same meaning to all normal people.

The division of common experience into metrical and non-metrical parts, of which only the former can be dealt with scientifically, therefore appears too simple. The whole of common experience is open to scientific treatment; part of that which is metrical is included in the physical scheme, and the remainder, together with the non-metrical elements, must be placed in a different scientific category—or perhaps more than one such category. And we must now notice that even this does not exhaust the potentialities of Science, for it has an influence outside its own proper sphere; namely, among those experiences which are peculiar to the individual. Such experiences are not in themselves subject to scientific treatment, but, by virtue of a parallelism which exists between them and experiences which are so subject, they cannot be considered as if they were altogether independent of Science; or, rather, if we do so consider them we are closing our minds to much relevant information. Let me give an example, which I think will show how the metrical and the non-metrical common elements, as well as the peculiar individual elements, may all be present in a single set of experiences.

I am unwell, and my medical adviser, by administration of a drug, restores me to health. Now there is, first of all, a purely metrical side to this process. My temperature is 102° F.; my pulse beats 120 times per minute. These are "pointer readings," as Eddington calls them—simple metrical quantities. At a later instant the world-line of the metrical structure which I call my body intersects the world-line of the array of quantum numbers vulgarly denominated "Dover's Powder." It is some time afterwards observed that the random motions of the molecules of my body correspond to a pointer reading of 98°·4 on the Fahrenheit scale of temperature, and the frequency of my pulse falls to 70 per minute. Here is a sequence of events accurately describable in terms of measure-numbers.

But there is another side to the matter. The interaction of the powder with my system is followed by certain movements and processes which are not included in the physical scheme. I am no physiologist, and cannot describe those movements and processes in the proper terms, but it is sufficient for our purpose to note that they are not deducible from the chemical properties of the materials

concerned which are represented by measure-numbers; they are peculiar to the association of those materials with what we call "life." Nevertheless, the processes are open to common observation; they have a certain "automatic" quality about them, so that my medical adviser expects them to follow from the administration of Dover's powder, but knows they would not follow from the administration of strychnine. They are, in short, open to scientific treatment and yet are not metrical.

Thirdly, there are my feelings. Before the arrival of the doctor I was uncomfortable, irritable, with no particular desire to remain in a world so obviously ill-constructed and mismanaged. Twelve hours later I am calm and indolently cheerful; it seems worth while, after all, to live a little longer, to learn a little more about the structure of the universe, to feel the wind on the heath, and even to see if England succeeds in regaining the Ashes. These feelings are individual; no one shares them. Another, nobler-minded, person, even in circumstances identical so far as the observable conditions go, would have maintained a more stable outlook. It is impossible to deal with such experiences in the scientific way. They exist only "in the mind" as we say, and have not even a ghost of a claim to be considered as part of an external world. Yet—and this is the point I wish to emphasise—they are not completely divorced from the scientific world. We cannot suppose that the change which takes place in them is only accidentally coincident with the administration of the powder. The two things are evidently connected in some way, and therefore we cannot limit the influence of Science to the domain of which she is mistress. What the nature of scientific influence in these other realms might be is not for present consideration, but I would point out that it is not necessarily a relation of cause and effect.

Before leaving this question of measurement, it is perhaps worth while to notice that the metrical elements of experience are not only entirely and unquestionably fit matter for Science, but are probably common to a wider range of persons than any other elements. Thus, a colour-blind person may not be able to appreciate the full subtlety of Swinburne's observation:

> Those eyes, the greenest of things blue,
> The bluest of things grey,

but give him a spectroscope and he will discriminate colours by wave-lengths a million times as finely as the eye of the keenest artist.

A deaf person cannot distinguish the horrors of modern dance music from the sonatas of Beethoven, but by the use of Lissajous's figures he can detect differences of pitch of which the ear of the most sensitive musician would be unconscious. Normality has a wider meaning in relation to metrical concepts than to any others. But it need scarcely be said that this by no means entitles those concepts to a monopoly of the realm of Science.

CHAPTER VIII

SCIENCE AND PHYSICAL DETERMINISM

We must turn now to the other question referred to on page 74—that of physical determinism. Modern physics has caused a considerable modification of our ideas on this question, and it has been necessary to suppose that the ordinary "cause-and-effect" relation, in which the effect always follows the cause, is not applicable to atoms and their parts. The relation has accordingly been dropped. Whether it will ever be reinstated is an open question, but there is a considerable band of physicists who think not—or, at least, who definitely hold the view that, whether it comes back or not, it is not a necessity of thought as we have hitherto supposed.

Before we examine the matter in more detail, I would like to repeat that the significance of this development depends largely on the view we hold of the status of the atomic scheme—a question we have already considered at some length (Chap. VI). If it is held that this scheme is the real truth about phenomena, then the significance of the new development is profound. It has been emphasised by Jeans and Eddington as of fundamental importance to our view of Nature and life as a whole. "Another striking change of scientific views," says Sir Arthur Eddington,* "is in regard to determinism—the view that the future is predestined, and that Time merely turns over the leaves of a story that is already written. . . . Until recently this was almost universally accepted as the teaching of science—at least in regard to the material universe . . . But to-day physical theory . . . is built on a foundation which knows nothing of this supposed determinism. So far as we have yet gone in our probing of the material universe, we cannot find a particle of evidence in favour of determinism." No reference is made here to the world of atoms; it is the "material universe" that is being discussed. Again, Sir James Jeans writes:† "The old science . . . gave us the illusion that . . . we should be able to determine both the position and motion of a particle at a given instant with perfect sharpness, and it was this illusion that introduced determinism into science. But . . . the new science . . . cuts away the ground on which the old determinism was based." We might perhaps pause for a moment

* *The Listener*, November 26, 1930. † *The Mysterious Universe*, p. 27.

to notice that actually this statement is inaccurate: the "old determinism" was based simply on the observation that phenomena were what was called "causally connected," and the "illusion" referred to was simply a deduction from this old determinism. But we will not linger on matters of history: the point is that this new development in atomic physics is regarded as telling us something about Nature which is truer than the conclusions we derive directly from phenomena.

Let us glance briefly at the reasons which have led to the transcending of the "cause-and-effect" relation in the investigation of atoms. On one hand there is the "principle of uncertainty" which we have already discussed. This imposes a limit on the precision with which the simultaneous position and momentum of an electron can be stated. We cannot, therefore, say that these quantities are "determined" in an exact sense. On the other hand, there is a group of phenomena which has led to the assignment to the atom of an "*a priori* probability," which means that when the atom has two or more courses possible to it, there is no sense in which we can say that it *must* take any particular one of them; we can only state the odds on its taking each. For example, an atom can exist in various "states," which are arranged step-like, and if it is in any state but the lowest, it must fall sooner or later to a lower one. Consider three such states, 1, 2, and 3, of which 1 is the lowest. An atom in state 3 may fall to state 2 or to state 1, but we cannot tell which it will choose. All we know is that, when we have a crowd of millions of atoms, a definite proportion will go to each state. This we know from observation, because each fall to a lower state is accompanied by the emission of a beam of radiation of definite intensity and wave-length. Each change of state has its own wave-length, and the total intensity of the radiation of each wave-length is a measure of the total number of changes of state of the corresponding kind. Thus, if the radiation corresponding to the 3–1 change is twice as intense as that corresponding to the 3–2 change, then twice as many atoms have fallen to state 1 as to state 2. We know of nothing which determines the action of a particular atom, but because of this statistical result we say that the probability that it will fall to state 1 is two-thirds, and the probability that it will fall to state 2 is one-third. Now probability is not certainty: it involves an element of indeterminacy, and so it is concluded that the atom is undetermined. There are other phenomena leading to a similar conclusion, such as

the explosion of radio-active atoms for instance, but no essentially different principle is involved in them, and the example I have given may be taken as typical.

Now let us consider what we mean by "indeterminacy." There are two meanings of the word, which we must clearly distinguish. First, when we say that a system is indeterminate, we may mean that its state cannot be described in terms involving only what has happened in the past. When we say that an eclipse of the Sun is determined for a certain date, we predict it entirely in terms of the positions and movements of the Earth and Moon before the eclipse takes place. We could not predict it if knowledge of events *after* the eclipse were required: in that case we should say that eclipses were not determined, for they would always come on us unexpectedly. To characterise this meaning of the word, I will call an event (such as an eclipse) which can be fixed entirely in terms of past events, a *predetermined* event.

But there is a second meaning of indeterminacy; namely, the quality of an event which is not describable at all in terms of other events, whether past, present, future, or timeless. Events indeterminate in this sense would be capricious, unrelated to one another, and therefore, to the extent to which the indeterminacy exists, intractable to Science.

The distinction between these two kinds of indeterminacy is important. I dislike analogies since, even at their best, they cannot avoid a liability to misapplication, but I will venture on an imperfect one here because I want to make this point as clear as possible. I must trust that the illustration will not be applied in too much detail. The first kind of indeterminacy is that of the actions of a man who abruptly begins to "scorn delights and live laborious days" because of the prospect of future reward. If we knew all about his past life, and nothing else, we should not be able to predict his actions, because they cannot be rationalised in terms of the past alone. Nevertheless, they are the reverse of capricious, and are definitely related to circumstances independent of him. The second kind of indeterminacy is that of the Hollywood film star, whose actions cannot be rationalised in any terms at all. If indeterminacy of the first kind exists in Nature, no looseness or lack of precision is involved; it simply means that there is a corner of Nature's workshop which she veils from our view; she keeps the secret of her manufactures until we have been shown the finished article. But if

indeterminacy of the second kind exists in Nature, then she has no workshop; all we can say is that the goods just appear.

Now I do not believe that either kind of indeterminacy exists in Nature; whatever indeterminacy there might be is in the conceptual world of atomic physics. But letting that pass for the moment, I think there has been a confusion of these two kinds of indeterminacy even in our view of what is happening in physics. It seems to me that all that has been introduced is the first kind, whereas it is sometimes assumed that there has been an introduction of the second kind. Thus, when Sir James Jeans writes:* "Heisenberg now makes it appear that Nature abhors accuracy and precision above all things," I think he is not only attributing to Nature a characteristic meant to apply to our own conceptions, but also that he is not justified in describing that characteristic in those terms at all. What Heisenberg has done is to transcend *pre*-determinacy in atomic physics; he has not questioned the supremacy of accuracy and precision.

Jeans's reference is, of course, to the principle of uncertainty, which we have already discussed (p. 62). Heisenberg's principle, as Bohr has clearly pointed out,† presents us with two alternatives: we can either persist in trying to fit the electron into a space-time framework and accept the unavoidable looseness of the fit, or release the electron from this bondage and retain the principle of strict causality intact. From our point of view the second alternative is clearly the one to be adopted. Apart from the principle of uncertainty altogether, we have seen that there is no *a priori* justification at all for requiring the electron to be describable in terms of space and time: the electron is not a potential phenomenon, and therefore is under no obligation to submit to the abstractions from phenomena. When we try, Procrustes-like, to force it to fit a time scale, it retaliates by lying partly in the future, and *the reason why it does so is that its actions are determined and it can do no other*. Heisenberg's principle not only reveals the inappropriateness of our action, but, by making that inappropriateness precise, gives us a clue to the conceptions, not yet formed, which would be appropriate. We have no right to condemn a sphere as an undetermined figure because a plane cloth will not fit over it without creasing. What we must do is, by studying the nature of the creases, to determine the precise figure of the sphere. It is not the most direct way of doing so, but since we are human and can think readily only in terms of plane surfaces (i.e.

* *The Mysterious Universe*, p. 26

† *Nature*, April 14, 1928.

space and time), it is in practice the only course that is open to us.

The interpretation of the other line of evidence for indeterminacy —namely, that of *a priori* probability, which I have exemplified by the probabilities of particular changes of state of atoms—is of precisely the same character. When we say, "the probability that the atom will change to state 1 is two-thirds," we are apparently stating something involving an inherent uncertainty; actually we are doing nothing of the kind. The phrase is simply a way of expressing the observed fact that two-thirds of the total radiation corresponds to this change. This is not only an observed fact; it indicates, also, accuracy and precision in Nature, for whenever we make the experiment we always find the same result. The phrase expresses this constancy, *and nothing more*: the "probability" assigned to the atom is simply a clue to that in the atom which *determines* the observation in question. So far from involving caprice, it symbolises the factor in the atom which excludes caprice. What that factor is we do not yet know, and in trying to express it in familiar but unsuitable terms we again find ourselves impelled to use words which suggest arbitrariness. Probability applied to an atom has a totally different meaning from probability applied to ordinary experience; the one is an imperfect expression of certainty, the other is a perfect expression of uncertainty.

I would like to emphasise a point which I little more than hinted at just now—namely, that this indeterminacy, however we interpret it, pertains only to the conceptual world of atomic physics and not to the world of observation—because, after all, that is the most important aspect of the matter for the non-physicist. No amount of theorising can alter the observed fact that there is a determinism in Nature. We predict an eclipse, and the eclipse happens; we apply heat to water, and the water boils; we throw a stone into the air, and it falls to the ground—all these things, and millions of others like them, happen with absolute regularity. Observations such as these are the parents of our theories, and the theories, however far they may wander, must finally return to sit again at the feet of the observations. In the latest developments of physics, of course, this is acknowledged. The obvious determinism of Nature is attributed to the mutual cancellation of the individual uncertainties of atoms for we can observe atoms only in very large numbers. It is a statistical effect, and the eclipse happens when we expect it, not because it must do so, but because it is too improbable that it will fail. Eddington

even appears to regard this high measure of probability as affording a greater degree of confidence in prediction than absolute certainty would do.

The regularity of our observations, then, is recognised and given a statistical interpretation, but what is often not clearly stated—and perhaps not admitted—is that this statistical interpretation is merely a form of expression. We are at liberty to adopt indeterminacy for atoms, but we must not abuse our freedom by extending it to phenomena. I have already quoted passages in which this has been done. The error, as it seems to me, is the reverse of that committed by the physicists of the last century. They unjustifiably foisted the characteristics of phenomena on atoms, and we are now witnessing the characteristics of atoms being foisted on phenomena. I think we must come sooner or later to the view that the question whether the failure of the eclipse is impossible or improbable is simply a matter of convenience of expression. Whichever view we take we are going beyond what is strictly contained in observation. Impossibility involves the introduction of a principle of compulsion, which can be justified, if at all, only metaphysically. Improbability involves the introduction of the atomic scheme of physics. As physicists we adopt the second, and we are on perfectly safe ground so long as we do not forget that our scheme of correlation is a voluntary addition to Nature. The philosopher may object to it as an arbitrary analysis of Nature into elements which inevitably hide themselves from view: we postulate an indeterminacy which is reconciled with observation only by assuming that we never get down to it. This again is certainly ...ry suggestive of the Red Knight's plan, but, after all, that plan ... not be so absurd as it appears. Green whiskers may have a ...y of preservation denied to those of brown, and in any case ...rer avoids the stigma, if stigma there be, attaching to the "greybeard." Similarly, the physicist's device allows the ... of a far greater range of observations than any alternative ... imaginable could deal with, and on this account he can ...philosopher's criticism with equanimity.

...e new view, it may be noticed, gives us a salutary reminder ...at the notion of "cause" has no place in Science. All that observation tells us on this matter is that certain states of a physical system invariably succeed certain other states. We go beyond this at our peril. We have no right to say that there is some hidden element, some "cause," in the first state which fashions the second; or, if we

do say it, we must perpetually remind ourselves that it is merely a mode of expressing the invariable sequence, and nothing more. "Cause" is like Newton's "force"—a term used as a conventional means of describing phenomena, and if we give it a significance beyond that we must sooner or later meet our Einstein, who will take it from us altogether. The modern quantum theory, in fact, is to the notion of cause what the relativity theory is to the notion of force.

A word must be said on the relation of the new physics to the question of human determinism. It is held by Eddington and Jeans that the recent developments of the quantum theory have changed the attitude of physics towards this question. I cannot, however, see that there is any change at all. The contribution of physics to the problem of human free will seems to me to remain precisely what it has always been—namely, nothing at all. Of course, subsidiary arguments from analogy with physical theory have been used to support the doctrine of human determinism. It has doubtless been urged that a strict determinism rules the behaviour of atoms, and since the brain is composed of atoms the mind also must be strictly determined. Such an argument naturally would break down if the behaviour of atoms were not strictly determined, but this slight fracture would be so small compared with the enormous cavity which has always existed between the brain and the mind, that it scarcely seems worth noticing. In the whole history of physics there has never been the slightest hint of a derivation of consciousness from the behaviour of atoms, cerebral or otherwise. On the contrary, we have seen that even the metrical aspects of life and consciousness—which are surely those most nearly allied to physics—are apparently hopelessly outside the physical scheme. There does not seem to be any advantage in discussing the details of that scheme in relation to human free will so long as this state of affairs exists. If any argument from analogy is valid in this matter it should presumably be drawn from the *macroscopic* equations of physics, because all known forms of life are polyatomic. An argument from this source would be completely unaffected by recent developments. But an argument from analogy between things whose dissimilarity is their most conspicuous characteristic necessarily stands self-condemned.

What I have been maintaining in the foregoing sketch of the history of physics is that the correlation of phenomena has been

conducted by the two processes of abstraction and hypothesis. In employing hypotheses we introduce a weapon excellent for its legitimate purpose but dangerous when misapplied. I have maintained that a hypothesis involving entities essentially unobservable is a pure conception, useful as an instrument but invalid as an object of veneration. The atomic hypothesis, with all its ramifications, is of this character, and the seemingly impossible requirements of the modern quantum theory have therefore a perfectly natural interpretation. The discovery that atoms and electrons are not to be clothed in the notions of space and time occasions no surprise; there is no necessity for conceptions to assume a garb abstracted from phenomena. In a world of reason, only rational elements must be expected.

It is always satisfactory to find a point of view from which what at first appears bizarre and occult takes its place in a natural order, and on this consideration the interpretation I have suggested seems worthy of acceptance. But the general view from the new platform must be considered also. Some violence is inflicted, on our prejudices at least, by the notion that the whole scheme of atomic physics, with its general laws and principles, is not a revelation of Nature but an instrument for making Nature intelligible to our minds. The physicist in particular must find it almost impossible to doubt that the hypothesis of atoms, though it may be imperfect in certain details, is fundamentally true. When we pass in mind through the various intricate phenomena of experimental physics, many of which have been discovered through the predictions of the hypothesis and seem to be inexplicable without it, we need all the courage and strength of reason we possess to assure ourselves that there may be other interpretations, in terms of concepts distinct from those of atoms, which may correlate the observations even better and include a wider range of phenomena. Yet there is no logical escape from this conclusion. It is a historical fact that atoms and electrons have been invented by the human mind to correlate phenomena, and they can never be observed. No proof is possible that they are the only, or even the best, conceptions suitable for the purpose, and we must therefore admit the possibility that they are not and that they will one day give place to others.

Whether such a possibility is likely to be realised or not is a question which we shall answer variously according to our predispositions and breadth of view. Those who restrict their outlook to

the group of phenomena studied in physics will think it a mere logical possibility devoid of any practical probability of realisation. Those, however, who are strongly impressed by the fact that dead and living matter appear physically identical and yet behave differently, may take another view. To them matter will appear to contain inherent possibilities which find no description in terms of the atomic hypothesis, and so long as that hypothesis remains as impotent to include those possibilities as it does now, they will not think its ultimate supersession an unlikely event. I know, of course, that the indeterminacy of atoms has been hailed as an indication that they may be capable of interpreting the phenomena of life. "The picture of the universe presented by the new physics," writes Sir James Jeans,* "contains more room than did the old mechanical picture for life and consciousness to exist within the picture itself." But I can see in this nothing more than a play upon words. Even apart from the confusion of the two meanings of indeterminacy which I have already discussed, it is a simple fact that the new physics throws not one single ray of light on the transition from the movements of the dead fly to the movements of the living fly. In that matter we are precisely where we were before, and the only change of outlook that has occurred is in the use of the same word to represent two different ideas.

Indeed, if it should be found later that the new physics *does* make clearer views on this problem possible, I think those views will be of the opposite kind to those drawn by Jeans. I have described the present indications of indeterminacy in atoms as clues to their character, which we must learn to interpret. But suppose we find an interpretation impossible, what will be the position then? We shall be left simply with undetermined atoms as our ultimate achievement, and the significance of this is extremely interesting. For we can recognise indeterminacy, I think, as the natural means by which a hypothesis can tell us when its possibilities are exhausted. We used to think that hypotheses must conform to the characteristics of experience, that they must be picturable as phenomena, but we know now that that was a false limitation; they are rational and not phenomenal, and so long as they afford a rational correlation of experiences we can dispense with graven images. But if we find ourselves forced to admit indeterminacy into them they cease to be even rational, and therefore necessarily become unfit to give us a

* *The Mysterious Universe*, p. 29.

rational correlation of phenomena. No further progress is then possible without abandoning the hypothesis for a better one, and all that is left to do with the old one is to work out its logical consequences.

Now let us suppose, for the sake of argument, that the thorough indeterminacy, including an abhorrence of accuracy and precision, which Jeans speaks of, is actually an established characteristic of atoms. Then the atomic hypothesis has reached the end of its tether. It shows not the slightest sign of interpreting life and consciousness, and so we shall have to transcend the atomic hypothesis by something more comprehensive before we can correlate the facts of biology with those of physics. We may not, of course, be able to do this. In that case we must conclude that Nature is at bottom irrational. Our most promising scheme of correlation—the atomic hypothesis—will have covered only a part of the whole field, and there will be a fundamental rift or rifts in Nature, separating that part from others. I do not, of course, suggest that we have reached this stage because, as I have explained, I see no reason for ascribing to atoms indeterminacy of the second kind, and, furthermore, what indeterminacy there is will in all probability lead us to the valid conceptions which it represents. But the possibility that it will not do so is, I think, a strong reason for believing that the atomic hypothesis may yet have to yield to something, at present inconceivable, of a still more general character.

If this should come about, will the beautiful structure of theoretical physics which now exists be simply destroyed, and all the labour which has been expended on it become a vain sacrifice? By no means: the development of Science proceeds not by destruction but by fulfilment. When the elastic solid ether gave place to the electromagnetic ether the change was not so much a substitution as a reinterpretation. Much of the old theory was taken over *en bloc*, and found a place in the new conception as prominent as that which it had occupied in the old. But it was reinterpreted: what was formerly looked upon as a material displacement was then regarded as a change of electric or magnetic intensity, and although the mathematical equations were the same, the physical idea was entirely different. In much the same way, there can be no doubt that in any wider scheme of correlation which may comprehend the characteristics of both animate and inanimate matter, the transformed atomic hypothesis will occupy an honoured place. When we reject

its claim to be the truth about Nature, we do not thereby characterise it as false. It undoubtedly *represents* the truth, but until we know which ingredients of it are destined to remain unchanged and which to be transformed, it is impossible for us to know the precise character of the representation. It is on this account that I disagree with those who draw fundamental conclusions about Nature from present ideas. When the whole of our common experiences are included in one grand generalisation, which proclaims itself indubitably as the only one of its kind which is possible, then and then only may we say that we have arrived at the truth about Nature. In the meantime we are on the march; let us not mistake the beauties of the wayside for the goal.

CHAPTER IX

COMMON AND INDIVIDUAL EXPERIENCES—SCIENCE AND ART

In the previous chapters I have discussed Science as a method of dealing rationally with experiences which have a certain quality; namely, that they are common to all normal people. That quality defines the area over which Science has legitimate sway, but it by no means exhausts the whole of experience. Each of us has experiences peculiar to himself, or perhaps shared by a limited number of others, which are just as vivid and enduring as those of universal scope. Let us consider one or two of these experiences as examples.

The religious experience is one of them. This is obviously not universal, and furthermore—a fundamental point, as we shall see directly—those to whom it comes never dream of rejecting it as an illusion because it is not shared by others; they evaluate it on quite other grounds than that.

> Yea, with one voice, O world, tho' thou deniest,
> Stand thou on that side, for on this am I.

There is no question about the intensity of the experience, and there is also no possibility of classifying it with the experiences correlated by the generalisations of Science.

Another such experience is that usually described as a "sense of beauty." Here again there is no universality: what appears beautiful to one appears ugly to another, while a third is altogether indifferent to its æsthetic appeal. Yet it was this experience

> that launched a thousand ships
> And burnt the topless towers of Ilium.

It cannot be ignored, but it is not susceptible to scientific treatment. A third example is the "sense of humour," which clearly has similar characteristics. Terror, morality, and other experiences which will no doubt suggest themselves, are of the same type.

Now so far as the individual who is subject to them is concerned, the separation of these experiences from the others may seem rather arbitrary. Some of them at least seem indubitably of the same character as experiences which are commonly shared; why should they be placed on the other side of the most significant line of

cleavage we are able to draw? The reason has nothing to do with the distinction between reality and illusion; in marking out the realm of Science, as we have seen in the Introduction, we make no assumption at all as to that. Nor is it concerned with the distinction between precision and vagueness. "He who does not imagine in stronger and better lineaments, and in stronger and better light," wrote Blake, "than his perishing mortal eye can see, does not imagine at all." The difference is simply that we have different criteria of existence in the two cases.

If fifty men are gathered together in a room, and one of them has a vivid experience of the presence of snakes of which the remainder are unconscious, we have no hesitation in saying that the snakes do not exist; the criterion of existence is the vote of the majority. Extend that to the whole human race and the whole of their experiences, and you have the field of Science defined. If the vote on a certain type of experience is unanimous except for a few dissentients, the dissentients are classified as "abnormal," and the unanimity of the "normal" people admits the experience in question to the category of existence. But now suppose that one, and only one, of our fifty men has a vivid experience of the presence of God in the room. We do not now say that God does not exist; many people, at any rate, would prefer to say that the forty-nine lack a faculty or appreciation which the fiftieth possesses. I am not concerned with the truth or error of that interpretation; I simply point out that, as a matter of fact, we can legitimately adopt a different criterion of existence from that applicable to the former case. The consequence is that the experience of God, whether more or less vivid, whether more or less enduring, whether more or less definite, than the experience of snakes, must be placed in an essentially different category before we begin to rationalise our experiences.

Why is it permissible to adopt these different criteria of existence? The reason is that snakes have previously been defined as belonging to the world of common experience; there is no divergence of opinion about the existence of snakes in the Zoo. But there has been no previous admission of God to the world of common experience. The religious experience is *always* an individual matter; even when it appears to be common to a limited group of people, there is no means of knowing whether it is identical in all of them. Hence there is evidence that the forty-nine are capable of experiencing the presence of snakes, but no evidence that they are capable of experi-

encing the presence of God in the sense in which the fiftieth experiences it. Consequently we cannot explain their blindness to the snakes by lack of the appropriate senses, and therefore have to admit the non-existence of the snakes. But we can so explain their insensibility to the presence of the God whom the fiftieth experiences, and we are not, therefore, justified in concluding that that God is non-existent.

If this point has been made clear, I may now proceed to explain that it has been inaccurately stated—or, at any rate, not stated in the most direct and valid way. What I should have said was, not that *we* adopt different criteria of existence, but that *the individual in question* does so. *We* adopt no criterion of existence corresponding to individual experiences, for the simple reason that since they are not general we cannot do so. For example, if I like tomatoes and you do not, it is idle to discuss whether that is because we both experience the same physical tastes but value them differently, or because we have a common standard of valuation but experience different physical tastes. The alternatives are only verbally different, because there is no conceivable means of deciding between them. In just the same way, it is idle to discuss whether God is in the room and only one man is conscious of his presence, or whether God is not in the room but one man is deluded into thinking that he is. Only the individual can answer that question in an intelligible way, and therefore no criterion of existence which can be *generally* accepted is possible. We may therefore amend our former remark by saying that common experiences, which are those amenable to Science, are experiences of which we can agree on a definition in terms of external existences;* individual experiences are those with respect to which we can reach no such agreement.

It is for this reason that attempts to define such postulates as Beauty, Goodness, Humour, and the like, have always failed; we have almost as many definitions as we have persons capable of expressing their individual experiences. It is inevitably so, and arguments as to whether such-and-such a statement on these matters is truer than others are vain babblings, because there is no means of ensuring that the disputants are talking about the same things. There is no meaning in asking whether a certain situation is "funny" or not, because the question implies a common standard of "funniness"

* This, of course, implies no answer to the metaphysical question whether there are external existences or not—see p. 19.

which does not exist. The situation may be funny to some and tragic to others, and the question, Which is right? is meaningless.

Why do we thus divide these individual experiences so arbitrarily into an external cause and a sensitive percipient? The emotion of humour is not accepted merely as an emotion; it is called "seeing the joke," as if there were an objective joke and a faculty for seeing it. Similarly, the æsthetic experience is an "appreciation of beauty," and the religious experience a "vision of God." Why is this so? The question is really one for the anthropologist or the psychologist or the philologist to deal with, but, speaking without expert knowledge, I would suggest that the attempt to objectify all our experiences may have been suggested by the proven value for practical ends of the objectifying of common experiences. From the days of animism onwards the practice has been universal. In the Middle Ages it was abused, as we have seen, by the assumption of occult qualities, and in modern Science it survives in the formulation of hypotheses. The practical convenience of referring an experience to some external source is so obvious that it would seem quite adequate to account for the universal adoption of the device.

But whether this is so or not, there is no doubt that the objectifying of these individual experiences has led quite unwarrantably to their discussion in terms applicable only to common experiences; that is to say, in scientific terms. We speak of beauty, humour, frightfulness, as if they had a reality of the same type as that of a chemical element. We analyse them, make categorical statements that they are present in this and absent from that, just as though they actually had a criterion of existence co-equal with that of the so-called material world. I think that one of the most urgent needs of modern thought is the abandonment of this vicious practice. By all means let us use the words in question for convenience of expression, if we can do so without attaching a scientific reality to them. If we are safeguarded from this danger, they have as much validity as the Newtonian "force" and the concept of "cause," which we have already considered. But the moment we forget that they are mere expressions of convenience, we become the potential victims of incalculable error, and the Einstein of metaphysics will have to lead us back along the path we have trodden to a truth which will outrage, not merely our common sense, as in the scientific sphere, but perhaps our moral prejudices also.

We recognise, then, a fundamental division of experiences into two classes. Those which are universal form the domain of Science; what activity of the mind can be said to reside in the others? I think they form the domain of Art, but in making this comprehensive statement I must guard against some possible misapprehensions. In the first place, I do not mean that the artistic treatment of individual experiences exhausts their significance. The religious experience, for example, has, of course, its own intrinsic value, and so have other experiences of this class; nevertheless, religion is legitimate subject-matter for Art. Secondly, although Art may appear to have some commerce with the realm of common experiences, that is never an essential part of its activities but only an incidental one. Thus, a painting—say, a landscape—certainly represents scenes which everyone can observe, but so does a photograph. The painting is a work of art only in so far as it represents elements which are not included in the untouched photograph, and those elements are always related to the individual, and not to the common, experiences of the artist.

Thirdly, although the two sets of experiences are sharply separated through the characteristics I have mentioned, they are not entirely independent of one another. The data of Science and Art are distinct, but the methods of operation have some elements in common. Let us recall our definition of Science—the recording, augmentation, and rational correlation of our common experiences. The artist seeks to express or record his experiences, and the work of art is the medium through which he does so. He seeks also to become more sensitive to such experiences—that is, he seeks to augment them. But he is not concerned with their rational correlation. If the object of his æsthetic experience is at one moment a tiger and at another a lamb, he does not look for the reason why two creatures naturally opposed to one another should excite the same emotions in him. He may ask of the tiger, "Did He Who made the lamb make thee?" but, like Pilate, he does not wait for an answer. Each experience is complete in itself; it has no need to stand in any relation to other experiences. The moment he begins to look for such a relation he ceases to be an artist and becomes a psychologist.

We have seen that the two great methods of correlation employed by Science are the methods of abstraction and hypothesis. These are evidently very fundamental instruments of the human mind, for they are employed also by the artist—not, as we have seen, for

correlation, but for the recording of his experiences; that is, for the creation of his works of art. On first thoughts it is somewhat strange that the same instruments should be employed in Science and Art for different purposes, particularly since the purpose served in Art is also one of the aims of Science. The reason is very interesting. In the Introduction I spoke of the division of consciousness into experiences—comprising sensations, emotions, and the like—on one hand, and reason on the other. Now for the purposes of Science these two departments of consciousness are quite distinct; the data are the experiences and the active agency is the reason. We commonly speak of the external world as the source of the experiences and the mind as the seat of reason. But in Art this sharp division is not possible. Many of the experiences of Art—the individual experiences—are associated with the external world, but many also are associated with creations of the mind, with ideas. For example, the religious experience may come from the contemplation of Nature—the same Nature as that which provides us with the data of Science—but it may come also from intellectual conviction, and is, in fact, almost invariably associated therewith. The experience of beauty comes from the vision of the material world, but it comes also from a poem and even an elegant mathematical process. Humour may be found in a physical situation, but it may be found also in a witty idea. There is a moral aspect of an action, but there is also a moral aspect of a thought. And so on, throughout the whole range of these experiences which I have called individual.

Now when the artist wishes to express an experience which comes to him from the physical world, he employs the method of abstraction, and when he wishes to express an experience which comes to him from the world of ideas, he employs the method of hypothesis. I do not know how general this rule is, but in poetry—which I will use throughout as an example of Art*—I think it is fairly safe to say that when it is not observed the poem is not of the highest quality. Let me give some examples.

Consider first the poet who experiences in the presence of Nature an emotion which he wishes to express. He is not concerned with

* It is very doubtful whether the various forms of Art have sufficient in common for the generic term "Art" to have much important universal application. It would therefore, perhaps, be safer to substitute "Poetry" for "Art" throughout what follows, although the restriction thereby introduced would certainly be too narrow.

the aspects of Nature which belong to Science—those which are observable by every normal person. They are, in a sense, in the way, and so he has to abstract from the scene before him what he conceives to be the source of his emotion and reject the remainder. Thus a process of abstraction is involved which does not exist for Science in so far as the mere *recording* of experiences is concerned, for the scientist takes all the experiences available which are of common quality. The only difficulty in this is that of observing minutely enough to detect them all; there is no difficulty in clearing off the dross of his individual experiences, if he has any, for they are immediately distinguishable.

Here are two stanzas which will exemplify the process of abstraction appropriate to such circumstances. In the first the poet is trying to express his experiences on hearing a farm girl singing in a field. The song arouses emotions, indescribable precisely in words, but suggestive of infinity, of something which embodies itself for the moment in the situation before him, but which is essentially ageless and illimitable, something which existed before the world was and which will go on when the world ceases to be. It all springs, of course, from a succession of sound-waves of definite pitch and intensity, but that is an aspect of the matter which the poet has to get rid of. He has to abstract from the scene that which may be conceived as arousing his emotion, and he writes this:

> Will no one tell me what she sings?
>   Perhaps the plaintive numbers flow
> For old, unhappy, far-off things,
>   And battles long ago:
> Or is it some more humble lay,
> Familiar matter of to-day?
>   Some natural sorrow, loss, or pain,
>   That has been, and may be again!

There is no indication here of the sequence of sound-waves, no record of the words of the song or of the "minims" and "crochets" which would constitute the "vocal score" of the song. These things would have answered the poet's question truly and completely, but they are irrelevant because the question is not a question; it needs no answer. The form of a question is employed simply to emphasise the sense of indefinability which is inherent in the emotion. And it will be noticed that no imagery is employed; it is not the poet's purpose to add to the situation but to subtract from it.

Or take this second example:

> Tears, idle tears, I know not what they mean,
>   Tears from the depth of some divine despair
> Rise in the heart, and gather to the eyes,
>   In looking on the happy Autumn-fields,
>   And thinking of the days that are no more.

Here again the poet tells us nothing about the actual scene—the fields yellow with corn, perhaps a bird singing in the distance and the Sun declining towards the west. The source of the emotion which comes to him is somehow wrapped up in these things, but he has to strip them off to get at it. And again you will notice that no imagery is employed. The farmer, on the other hand, who at the same time is looking on the happy autumn-fields and thinking of the cash they represent, has no use for the poetic abstraction. He has no need, however, to isolate and discard it because it does not enter his experience at all. What he is concerned with are the area of the fields, the size of the grain, and the correlation between the weight of corn and coins of the realm—all part of the subject-matter of Science.

These quotations exemplify the expression of individual experiences derived from the contemplation of Nature. Now let us turn to individual experiences which arise from the contemplation of ideas. Here abstraction is out of place; it cannot be employed without reducing the poem to a formal doctrine or a platitude. What is necessary is imagery, which is the equivalent of a hypothesis—something additional introduced into the situation to enable us the better to express and appreciate it. Take the moral idea for example. This cannot be represented in poetry by abstraction; if we attempt it we get such results as Longfellow's *Psalm of Life*, which should be preserved as a perpetual warning to poets not to tread the path of direct statement in the world of ideas. But listen to this:

> Last,—if, upon the cold, green-mantling sea,
> Thou cling, alone with Truth, to the last spar,
>   Both castaway,
> And one must perish,—let it not be he
>   Whom thou art sworn to obey!

We are on a different plane entirely. The infusion of an extraneous element into the idea reveals in it a quality which would otherwise remain hidden, and turns it from a platitude into a poem. Words-

worth, so direct and simple when writing of the Highland girl's song, overflows with metaphor when he writes of Duty:

> Stern daughter of the voice of God!
> O Duty! if that name thou love,
> Who art a light to guide, a rod
> To check the erring, and reprove;
> Thou, who art victory and law
> When empty terrors overawe;
> From vain temptations dost set free;
> And calm'st the weary strife of frail humanity!

Another example, which at the same time illustrates pointedly the indifference of the poet to correlation, is provided by Fitzgerald's "Omar," a philosophical poem which is packed with imagery from beginning to end. Consider the first stanza:

> Awake! for morning, in the bowl of night,
> Has flung the stone that puts the stars to flight,
> And lo! the Hunter of the East has caught
> The Sultan's turret in a noose of light.

Here, in four lines, we have two distinct metaphors of the same thing; sunlight is at first a stone and then a noose. Considered as Science, these metaphors would be hypotheses of radiation: compare them with the two pictures of the electron as a particle and as a wave. The scientist is forced to reconcile these pictures before he can employ them, because he seeks correlation of his phenomena, and since he cannot do so he deprecates mental imagery altogether and recommends attention only to the mathematical formulæ. But the poet is not concerned with correlation and does not trouble to reconcile his images. He leaves them both in the poem, mutually exclusive though they are, and if we think that is a defect of his work we have failed to understand his purpose.

We must look upon Science and Art, then, as essentially different activities, between which, nevertheless, a sort of parallelism exists. Science can have no authority over the artist because the experiences treated in Science are fundamentally different from those which form the subject-matter of Art. If the direct practice of Art were the only operation performed on our individual experiences, their introduction into a discussion of the scope of Science could be justified only by the plea that a region might be delineated by describing what is outside it as well as what is inside. But there is, in fact, a stronger plea than that, arising from the existence of criticism. Let us, then, see what Science might have to say in relation to criticism.

CHAPTER X

SCIENCE AND CRITICISM

I HESITATE to give a definition of criticism because it is a perpetual source of disagreement among critics themselves, but I think it is safe to say that it is very largely, at least, of the character of a science of Art. Restricting ourselves still to the example of poetry, we may say that the raw material considered by the critic is a collection of common experiences, namely, poems. These are presented to all critics alike, and it is the critic's business to evaluate them according to some standard or standards. This evaluation is simply a correlation of the poems, for their relations with a common standard express also their relations with one another. The critic's task, then, is the correlation of common experiences, which is precisely the task of the scientist.

It may be objected that although the raw material considered by the critic may consist of common experiences, what he is really concerned with is an individual experience, a quality abstracted from the poem which an insensitive person would not detect. But to urge that is to beg the question. The poet may write merely to express his own emotions, but the critic must necessarily write for others. He may abstract an individual experience from a poem, but in so far as he does so he is an artist, albeit an inarticulate one. As a critic his business is to give an objective estimate of the poem. Merely to state his own impressions is not sufficient; he must defend those impressions by relating them to some clearly defined standard. He must say that the poem is of interest in a certain respect because of such-and-such a characteristic, or not of interest in that respect because of such-and-such another characteristic. If it be argued in reply that the "respects" and "characteristics" in question are not related to common experiences, and that the critic writes only for those who are sensitive to poetic value, this must be admitted to be true—it is, in fact, what I shall presently maintain with an alteration in phraseology—but the fact remains that the overwhelming majority of critics write as though, within the limited company "sensitive to poetic value," the experiences in question are of the same nature as the common experiences dealt with in Science. They assume that "poetic value" means the same thing to all. Its existence is therefore to

be tested by the same criterion as that which we apply to our common experiences, and therefore the general principles regarding that criterion which have been found useful in Science will be applicable also for the benefit of criticism. What I shall try to show is that the application of those principles leads us to the conclusion that the assumption of community of character between the experience of poetic value and the common experiences of Science is a false one and is responsible for much of the confusion of criticism.

As a representative of literary criticism I choose, without any apology, Matthew Arnold. Like other thinkers and writers of whom legitimate appreciation has developed into uncritical reverence, Arnold is now suffering from the inevitable reaction. Modern criticism takes little account of him. Either by disparagement or, more frequently, by neglect, he is in effect removed from the history of criticism, and modern developments show—or rather acknowledge—little of his influence. This, though in a sense regrettable, is only what might have been expected, and is perhaps as inevitable as his ultimate restoration to a saner and more stable recognition. We are familiar enough now with this phenomenon of reaction in popular and expert appreciation, and no wise spectator will interpret the neglect of the moment as the final verdict. It is not, however, only, or even mainly, on account of his merit that I choose Arnold, but rather because he has expressed as clearly as anyone a view of criticism which, in spite of their occasional superciliousness, modern critics share. I will therefore advert to his famous essay, "On the Function of Criticism at the Present Time," written in 1864, and if I should happen to discuss it on a rather wider basis than that necessary for my immediate purpose, I trust the interest of the subject will be accepted as a sufficient excuse.

Although the essay in question affects to deal with the position of criticism in the year 1864, there is more than a hint that its contents have no such temporal limitation. Arnold set himself, in fact, to define what criticism actually is and what are its necessary relations with creative power. "The critical effort," he says, is "the endeavour, in all branches of knowledge, theology, philosophy, history, art, science, to see the object as in itself it really is." Its function—so far, at least, as literature is concerned—is to create an atmosphere of "the best ideas on every matter which literature touches, current at the time." Only in such an atmosphere can the creative spirit operate. Two powers must concur for the creation

of a masterpiece of literature—the power of the man and the power of the moment—and the power of the moment is wielded by the critical faculty. By its operation "it tends, at last, to make an intellectual situation of which the creative power can profitably avail itself. It tends to establish an order of ideas, if not absolutely true, yet true by comparison with that which it displaces; to make the best ideas prevail. Presently these new ideas reach society, the touch of truth is the touch of life, and there is a stir and growth everywhere; out of this stir and growth come the creative epochs of literature."

In a later essay, on "The Study of Poetry," prefaced to Ward's *English Poets*, he defines more specifically what he means by "the object as in itself it really is." He sets against the "real" estimate of poetry the "historical" and "personal" estimates, according to which a poem is evaluated in terms of its historical importance or its relation to qualities or circumstances peculiar to the reader. These latter estimates are fallacious and must be avoided. "In poetry the distinction between excellent and inferior, sound and unsound or only half-sound, true and untrue or only half-true, is of paramount importance," and this distinction resides in poetry "as in itself it really is," apart from the circumstances of its production or the relation it bears to a particular reader. The task of the critic is to make this distinction, ruthlessly and uncompromisingly.

Before proceeding to the direct bearing on this matter of modern scientific ideas, I may perhaps be permitted to make a few general comments on Arnold's contribution to the theory of criticism. In so far as his work is a plea for disinterestedness in criticism, no one will dispute its value. We have in large measure achieved the ideal at which he aimed in this respect, and it is now difficult to conceive of a time when the general estimation of literary value was determined by the relation between the political or religious ideas of the writer and those of the critic, however much the practice may linger among those impervious both to precept and to example. For this we have chiefly to thank Matthew Arnold.

But his work in this direction is done. It was the means—or one of the means—by which he served his age, but it is of little service to ours. More important for our own time is another corollary of his definition, which he does not mention but which he would doubtless have acknowledged. The object "as in itself it really is" is not necessarily the object as the author meant it to be, and the chief concern of the critic is with the former. Criticism greatly

occupies itself with the task of discovering an author's purpose, and estimates his work by the quality of that purpose or the degree in which it is achieved. "It will scarcely be denied," says Mr. Scott-James, in his excellent work, *The Making of Literature* (p. 273), "that the first business of the critic is to endeavour to put himself at that view-point," (the view-point of the artist) "to see the work of art from within and without as nearly as possible as the artist saw it." There can be no doubt that the endeavour to reach the artist's view-point is important work, but it is not the primary function of criticism. Strictly speaking, it is not criticism at all, but psychology. True criticism may find the highest value of an object in some element of it of which the author was unconscious and which he would perhaps have eliminated had he been aware of its presence. I do not know whether Dodgson intended *Alice in Wonderland* to be only a delightful phantasy or a highly symbolic allegory, nor, so far as my appreciation of the work is concerned, do I at all care. Doubtless the answer to the question would be valuable knowledge, but it would be knowledge about Dodgson, not about the book.

Arnold's work, then, has at any rate some significance for our time. On the other hand, we can see also that it stands in great need of modification. Thanks to the intellectual progress of this century, a new light shines on the problem of men's relation to the universe and to the thoughts and actions of other men, and in that light defects in Arnold's outlook, some of which were doubtless discernible in his day, become obvious. We can no longer allow the critic to scorn the historical or the personal aspect of his material, particularly when he is dealing with current work. The significance of, say, *Journey's End* to our generation is at least as vital a matter for the critic to consider as its meaning for posterity. And in the consideration of current work by a young writer, the estimate of the object as in itself it really is should certainly yield pride of place to the estimate of the object as an indication of what might be expected in the future. One thinks of Keats's *Endymion*, and wonders why Arnold did not realise this limitation of his dictum.

Further, in setting the critic the task of selecting and disseminating "the best ideas on every matter which literature touches, current at the time," Arnold is clearly proposing a hopeless task. In his day it might have been possible to a genius, but in our day it is impossible to a god. Ideas are not merely current; they are rampant. There is no need to form them into an atmosphere; the atmosphere is already

here, so dense as wellnigh to stifle us. The causes of this condition are no doubt manifold, but one thing which is perfectly clear is that criticism is not among them. Our mental, like our physical, atmosphere has many constituents—philosophical, religious, political, literary, artistic, and what not—but the vital portion, the "oxygen" of the mixture, is the scientific element. We live in a scientific age intellectually no less than materially. And the propagation of modern scientific ideas has been the work, partly of the creators themselves and partly of their entirely uncritical expositors in popular books and articles: the critic, properly so-called, has contributed nothing.

This, of course, was inevitable, as we can now clearly see. Very few but specialists can understand modern ideas, much less select the best. Nay, even criticism itself has fallen among psychologists, metaphysicians, and the like, and the critic as Arnold knew him may pass by on the other side for all he can do to save it. This is no doubt all to the good. One cannot read modern works of systematic criticism without feeling that there is something in them more stable, more in touch with reality, than the incurious, dogmatic reliance on taste and authority of a generation or two ago. There is the danger, it is true, of loss of sensitivity to artistic value, but it is at least doubtful if that is so vital for the critic who examines the principles of his *métier* as for the critic who judged new work by comparing it with old.

But to come to the main subject of our consideration, the fundamental defect of Arnold's view, when examined in the light of present-day ideas, is its extreme objectivity. I have already referred to this characteristic of our expression of individual experiences in general; we have now to consider it more closely in a particular case. The phrase, "the object as in itself it really is," implies the existence of some artistic value, not ourselves nor dependent on us in any way, which characterises the work of art and by virtue of which the work of art must be judged. That this is Arnold's view he leaves us in no doubt whatever. The quality by which the work is to be estimated is, of course, not technical excellence, grammatical accuracy, or anything for which more or less universally recognised external canons undoubtedly exist, but a much more subtle quality which I may refer to as "poetic value" for my immediate purpose and which may be described generally as "artistic value." This quality is detected and appraised by a faculty called "taste," or "artistic sensibility," or "appreciation," which bears the same relation to artistic value as the eye bears to shape or colour. An object has

artistic value independently of whether that value is observed by the reader or spectator. If he does not perceive it when it is there, his perception is at fault; if he perceives it where it is not, he is the victim of an illusion. The objective existence of artistic value is absolute. In other words, artistic value is potentially a common experience.

Now we have seen (p. 63) that one of the most significant movements in modern physical Science is the recognition that we cannot profitably employ in thought, any idea whose precise character cannot be observed or deduced and expressed in terms which can claim common assent. The absolute velocity of a body, it will be remembered, was one such idea, and fundamental progress in mechanics was found to be impossible until it was rejected. Let me direct particular attention to the words, "in terms which can claim common assent." Anyone can determine his absolute velocity on the assumption of absolute space and time, but when the results obtained by different observers are compared, they are found to be contradictory. Each observer finds that he is absolutely at rest, although he may obviously be moving with respect to the others. No reconciliation is possible until it is recognised that absolute velocity is merely a phrase, with no corresponding reality.

The principle thus established was first caught sight of in physics, but it is clearly applicable throughout a wider domain. Any idea which claims to refer to an actual or potential common experience is subject to it, and the idea of artistic value answers to this description. Before the claim can be substantiated, therefore, a common standard of artistic value must be recognised and accepted, at least by all who claim to be susceptible to this characteristic of Art.

When we examine artistic value, however, we find that such a standard appears to be inaccessible; it has all the characteristics of the phantom. There is proverbially no disputing about matters of taste; everyone is a law unto himself. If we disagree with a member of a certain African tribe about the beauty of women with artificially distended lips, we must be content to differ; there is no independent standard by which we can consent to decide the matter. Arnold himself recognised the need for such a standard, and attempted to provide it for poetry by indicating a number of "touchstones," as he called them—lines which he regarded as containing the highest poetic value. "If you are in doubt about the value of a poem," he says in effect, "compare it with these examples: you will see at once whether

it belongs to the highest class or not." The method is simple, but when we search it for the essential characteristic of absoluteness, what do we find? Mr. Scott-James writes of one of the touchstones:* "What are we to say of these indifferent lines with their redundant phrases? Milton morally exalted—poetically, in his worst vein." Miss Rebecca West contents herself with saying† generally that Arnold "was a man of very poor taste," while Miss Edith Sitwell‡ pronounces a condemnation which is indicative more of passion than of judgment and therefore need not be quoted. It is clear that there is no standard of artistic value to be found in "touchstones," nor has any more promising medium yet been suggested.

We are forced to the conclusion, then, that "the object as in itself it really is" is a meaningless phrase, unless we are prepared to save its significance by making it puerile; apart from that we can make it significant only by transforming it into "the thing as it is in relation to an observer." A poem "as in itself it really is" is a collection of marks on paper; a poem as it is in relation to a young child is a succession of more or less difficult words; a poem as it is in relation to some men is an indirect way of saying something not worth saying; a poem as it is in relation to others is a poem. It is only the "collection of marks on paper" that is capable of winning universal agreement, and that is all, therefore, that the poem is in itself.

Naturally, with the abandonment of objective artistic value we must cease to judge the perceptive faculty of "taste" by reference to the artistic value of the objects in which it takes pleasure. If we continue to speak of good and bad taste, we must assign the values on other grounds than those of the essential characteristics of the works of art contemplated. In so doing we relieve ourselves of many difficulties and contradictions. How otherwise, for instance, are we to explain the fluctuation in the successive historical estimates of some of our greatest artists, or the conflicting opinions we form of almost every critic when we consider his judgments as a whole? This anomaly is an old one, and is so well known that examples are superfluous, but it has never been and can never be removed so long as taste is related to an objective artistic value.

The difficulty which has always been felt in denying absolute standards of artistic value and taste is that in doing so we come peri-

* *The Making of Literature*, p. 278.

† *Tradition and Experiment in Present-day Literature*, p. 192.

‡ *Ibid.*, p. 97.

lously near to denying artistic value altogether. If Shakespeare's work has no definite quality absent from Tupper's or present there in much smaller degree, is it not meaningless to say that Shakespeare is a greater poet than Tupper? The difficulty is obvious, but it is not necessary. In denying absolute motion we do not deny motion itself; the advance from Newton does not lead us back to Zeno but forward to Einstein. There is a very real difference between the scholar's appreciation of Homer and the domestic servant's absorption in the adventures of Ermyntrude as they are recorded in successive numbers of *Dolly's Weekly*. The task which faces us is to express that difference in terms of the appreciations themselves, without invoking a hypothetical literary quality in the corresponding objects. If we can select some characteristic of literary sensibility by which this can be done, that characteristic will be our substitute for Arnold's "object as in itself it really is," and we may then perhaps take over his contribution to criticism almost completely, with advantages as vital for us as for the nineteenth century.

To be thoroughly consistent, I should speak only in terms of our experiences, without regard to anything that may be conceived to have caused them. This could be done, but the difficulties of language and customary habits of thought are so great that by doing so I should incur great risk of unintelligibility or misapprehension. I will therefore go half-way to meet the purely objective view I wish to displace, and speak of "relations" between the work of art and the spectator. Since all that the spectator is conscious of is that end of a relation which is in himself—i.e. his own experience—it is clear that translation into the language of experiences alone is possible throughout. Let us say, then, that when we recognise an object as a work of art, certain relations are established between it and us. "Relations" here means what is often spoken of as "communication," but inasmuch as that word may be held to imply a conscious intention to communicate on the part of the artist, and is liable further to connote some definite objective entity which is communicated, it is preferable, though less picturesque, to use a more abstract term. These relations depend for their character both on the work of art and on the spectator, for all works of art are not equally appreciated by one spectator, nor is each work of art equally appreciated by all spectators. Hence, in the last resort, each individual's relations with a particular work of art are peculiar to himself. This is, in fact, merely another way of saying, as we have

already done, that Art is concerned with individual experiences. At the same time, since appreciation of Art is not entirely capricious, we may assume that there is something common to the relations characteristic of different spectators, and we may (confining our consideration still to a single work of art), as a first broad classification, speak of *general* and *particular* relations, the former being identical for many persons, and the latter characteristic of individuals. It must be understood, however, that these general relations are not necessarily associated with experiences of the same quality as the common experiences of Science, for we have not the same assurance that what is experienced by the different spectators is identical. We are confident that the element *iron*, in the scientific sense, is the same to one observer as to another, but we do not know that two admirers of *Hamlet* have the same experiences on reading the play. We have to assume that, in order to rationalise criticism, and that is what we are doing now.

General relations may, of course, be identified differently, according to the basis of selection. Thus, they may be common to people of a particular country, or to those living at a certain time or experiencing similar conditions of life, the most general being those which are most independent of changing circumstances. In the realm of literature, the ancient classics will at once spring to mind. The particular relations are, of course, ultimately peculiar to individuals although usually they may be said to be shared by a small group. They are not sharply divided from the general relations, because even a small group involves some generality, and the distinction between general and particular, though serviceable, is therefore somewhat vague. More comprehensively, we may say that the relations between spectators and works of art can be characterised by their varying degrees of generality.

It is tempting to generalise this conception and extend it to the whole of our experiences, common or otherwise. Thus, if we classify the relations established with the world through the five senses into general and particular relations, we find that all normal persons have only general relations, corresponding to the common experiences dealt with by Science. We may then, perhaps, explain the identity of the sense relations of the great majority of people in terms of the usefulness of common conceptions in the struggle for existence. Art appreciation is practically useless for survival purposes, and therefore Art relations are far from being completely general. The

fact that, although useless, they have become so general as we find them, is perhaps evidence that the æsthetic faculty is not *sui generis*, but is an offshoot of the fundamental structure of our minds. Furthermore, the experiences of persons such as lunatics and visionaries, which we have had quite arbitrarily to exclude from the field of Science, at once fall into line as corresponding to particular relations, and the only distinction between the data of Science and those of Art is that in the former there is a far greater predominance of general relations than in the latter. These considerations, however, though of considerable interest, are not necessary to the validity of our view of appreciation of Art. There is, in actual fact, undoubtedly a sharp distinction between the most general of our relations in the world of Art and the common experiences examined by Science.

According to the view which I am suggesting, the function of the critic is not to look for unmeaning absolute qualities, but to classify a work of art according to the generality of the relations which it will set up. The critic will distinguish different types of generality, such as those associated with particular places or times. He will not be precluded from taking account of particular relations, but he must note that they are particular. He will not be expected to abstain from considering historical or personal relations, which Arnold deprecated—indeed, he will be required to consider them—but he must note that they are historical or personal. What will be prohibited to him, however, is the statement that this is "better" or "worse" than that, except as a synonym for a greater or less degree of generality of the same kind. Anything, in fact, which presupposes an absolute "artistic value" in the object of his criticism will be outside his province; everything which indicates the character of the experiences which the work will arouse in the spectator will be legitimate material for his analysis.

It is not part of my intention to show how the critic may proceed with his task; indeed, it would be presumption on my part to attempt to do so. I am merely suggesting how the nature of that task may be reinterpreted to meet a situation which the recent advances of physics have created. Nevertheless, I may perhaps fairly be expected to give some sort of indication of the kind of factors which determine the generality of the relations in question. I will therefore briefly mention two such factors: namely, custom and knowledge.

Some degree of familiarity with an object appears to be necessary

before we can establish æsthetic relations with it. The completely novel is rarely welcome, and what appear to be abrupt changes in literary form and taste can almost invariably be traced back through more or less protracted periods of preparation. As an example of æsthetic indifference to the unfamiliar I might mention a spectrum. There are persons, certainly not insensitive to beauty of customary kinds, who see no beauty in a perfectly defined line or band spectrum on seeing it for the first time. It is incredible, but it is unquestionably true. This has nothing whatever to do with knowledge of spectroscopy. The fact that the spectrum represents magnesium or cyanogen is quite irrelevant, and the spectroscopist is as charmed before he begins to analyse it as afterwards—more so, in fact, if the spectrum happens to be an intractable one. The critic must allow for the influence of custom in estimating the degree of generality of his works of art.

But custom may stale, as well as prepare, the mind for the æsthetic relation. This may be due to irrelevant associations as well as to excessive familiarity. One of the most striking examples is provided by the detestable modern practice of naming novels by phrases filched from great passages of English literature. It is impossible, after seeing and hearing these phrases constantly, day after day, in their new associations, to read them in the original settings with the old appreciation; a few years ago no one could think of the concluding lines of Shelley's magnificent "Ode to the West Wind" without the intrusion of a foreign and inferior idea. I find it very difficult to believe that authors who practice this type of defamation have any appreciation of literature. They would presumably view with disgust the tarring and feathering of statuary: why, then, should they outrage their own craft in a similar manner unless they are insensitive to its delicacy? In this, perhaps, lies the one redeeming feature of the business, for the vast majority of the novels so named are very ephemeral phenomena. But the practice shows no sign of abating, and there is not much consolation in resuming the old relation with one masterpiece when another is detached to take its place. Here is a direction in which the critic might well find work for his hand.

The second factor which I mentioned just now as influencing the generality of our relations with works of art was knowledge. The most obvious example of this is, of course, language. A poem cannot arouse in us any artistic emotions unless we understand the language

in which it is written—and, as a rule, not only understand but are familiar with that language, so that the words and phrases awaken in our minds recollections, not always conscious, of former associations. A poet—such as Father Hopkins, for example—who uses language in an unfamiliar way, will establish less general relations, other things being equal, than one who writes in familiar terms. The critic will note this, and he will estimate the class of readers who will be repelled by such a poet and the class to whom perhaps the difficulty will be an added delight. He will recognise also that a poem may lose generality of appeal by going too far in the opposite direction—that is, by clothing itself in language which is too familiar; there are many who find the most distorted lines of Hopkins preferable to the easy colloquialism of Masefield's "Everlasting Mercy."

Many other ways in which knowledge can influence artistic appreciation will no doubt suggest themselves—for example, a knowledge of the classics in reading Milton—but I will consider only one of them, which happens to be closely related to our main theme—namely, a knowledge of mathematics. The mathematician who has any artistic sensibility at all cannot be unconscious of the varying responses he makes to mathematical processes, quite irrespective of their truth. "Beautiful" and "elegant" are adjectives in common use in the literature of mathematical physics, and if "ugly" and "clumsy" are less frequently met with, that is simply because mathematical physicists as a class are very polite people. But before these descriptions can mean anything at all, the reader must understand the symbolism and operations of mathematics; otherwise he will experience nothing, except perhaps an instinctive revulsion. Not long ago I heard one of our leading poetesses comment on a passage in Sir Arthur Eddington's book, *The Nature of the Physical World*, in which he contrasts the representations of water waves by the hydrodynamical equations and by the sestet of one of Rupert Brooke's sonnets. I forget the precise terms in which she characterised the equations, but if I put the words "fearsome" and "terrifying" into her mouth, I shall express her idea without erring on the side of overstatement. Yet Eddington, to whom mathematics is as familiar as the alphabet, writes thus:* "I know of passages written in mathematical symbols which in their sublimity might vie with Rupert Brooke's sonnet." There is no question here of superiority or inferiority of æsthetic sense; it is entirely a matter of

* *The Nature of the Physical World*, p. 320.

knowledge. The critic, if he is to do his task thoroughly, should possess this knowledge; he would find a virgin field for his activities, and he would not be hampered by the requirement, which is absolute for the mathematical physicist, that the processes he discusses shall fit into the general scheme of scientific correlation—in other words, he would be concerned only with the beauty, not with the truth, of the processes. I do not, however, anticipate that critics will be impatient to avail themselves of this opportunity.

There appears, in fact, to be no class of objects or ideas, from the blessed word, "Mesopotamia," upwards, with which the human mind is incapable of establishing an æsthetic relation. The task of the critic in its completeness is impossible. He must restrict his considerations—as in practice, of course, he does—to a limited department of knowledge, making no attempt to deal with "every matter which literature touches." Even so, he cannot hope to do his work thoroughly. To determine precisely the generality of the relations which may be set up with a work of art, even of a particular kind, with respect to nationality, individual opinions, the lapse of time, and what not, needs a complete knowledge of psychology—and, in fact, it is difficult to see how the critic can even begin his work without being to some extent a psychologist. "Nobody," wrote Mr. Herbert Paul of Matthew Arnold, "after reading *Essays in Criticism*, has any excuse for not being a critic." If we substitute the function of criticism here proposed, we may say rather that no one has any qualification to be a critic.

Yet there is not so much change, after all. Mr. Paul's words refer to Arnold's illustrations of how to criticise rather than to his general idea of the function of criticism. That idea imposes difficulties on the practical critic no less great than those imposed by our suggested substitute, and I think we can directly relate the two ideas if we regard "the object as in itself it really is" as corresponding to the *persistence* among mankind generally of the æsthetic relation which the object sets up—that is to say, to the degree of generality of that relation with respect to time. I think Arnold would have agreed that what he called the "best ideas" were those which retained their power of exciting the æsthetic relation for the longest periods of time. A modern "best seller" makes a more general appeal than the *Iliad* if we consider only the moment, but I do not think that anyone who attaches absolute values to literature would consider generality in space as comparable in importance with generality

in time. If, then, we reinterpret Arnold's "object as in itself it really is" in the way I have suggested, we can profit by his precept and practice without the fear that they stand on an insecure philosophical foundation. His method of "touchstones," for example, becomes an excellent practical device for the critic, for what has defied time in the past may clearly be regarded (if we know how to apply it) as a key to the knowledge of what will persist in the future. But the touchstones must no longer be chosen to satisfy the individual peculiarities of the critic. They must be passages on which there is general agreement among critics of various places and times.

And this brings us to another advantage of the new conception; namely, that it allows of a greater measure of self-reliance in the individual critic than does the old. If we attach an objective artistic value to a work of art, the aspiring critic must naturally assume that the established critics of his day or of the past will have properly assessed that value in the objects of their consideration. He will accordingly tend to train his judgment to conformity with theirs instead of preserving his freedom to establish his own relations with literature. A young critic of Arnold's day, for example, who happened to be unable to agree that the line, "Fallen cherub, to be weak is miserable," was a standard by which all future poetry could properly be tested, would have been in an awkward predicament. He would have had either to believe his judgment superior to Arnold's, or else to modify his taste by an *ad hoc* discipline until he became merely an echo of his master. This difficulty is inseparable from any view of criticism which hypostatises artistic value. If, however, it is recognised that any critic's response to a work of art is made up of particular as well as general relations, the critic is free not only to develop his individual reactions, but also to form a criterion for distinguishing those individual reactions by actual comparison of his sensibility with that of others—an objective process which has none of the arbitrariness associated with dogmatic ascriptions of artistic value.

This must inevitably make for greater honesty in criticism. I do not suggest that present-day critics are consciously dishonest, but I think it is extremely probable that some of them have certain peculiarities (shall we say, for example, a dislike of Milton?) which they prefer to keep secret. The admission of such a peculiarity would inevitably be accompanied by a lowering of the critic's reputation in the eyes of others without that peculiarity, and he might

even persuade himself into a false appreciation of the poet in question rather than acknowledge so shocking a defect of taste. But if we disavow all belief in a faculty of taste, the incentive to such a perversion would naturally disappear. Another of the many problems which would then appear in a new light is the rather ridiculous discussion, "lowbrow *versus* highbrow," but we need not spend time on that.

What I have tried to do in this chapter is to show that Science may have an influence, and a valuable influence, in non-scientific spheres of thought. I have merely given one example, selecting criticism for the purpose, and I hope I have been able to show that the assumption of objective artistic value, hitherto generally made, must necessarily be abandoned; the criterion of objective existence derived from physics makes that imperative. I do not claim that this could not have been seen without scientific knowledge; if it is true we may expect that many roads will lead to it. Indeed, it has been arrived at quite independently of physics by some modern critics; Mr. I. A. Richards, for example, makes it the foundation of his recent theory of criticism. But that in no way detracts from the importance of considering the progress of Science in relation to these matters. Mr. Richards apparently does not realise that physics provides him with the strongest argument for his case, for he writes:* "No one of our recent revolutions in thought is more important than this progressive rediscovery of what we are talking about. One current in this change is towards toleration, another towards scepticism, a third towards far more secure founding of our motives of action. The startling philosophical changes in the general outlook sometimes predicted for Relativity . . . appear likely, if they occur at all, to be engulfed by these more unobtrusive but more domestic changes." If the previous chapters of this book have succeeded in their aim, it will be clear that the miscellaneous changes which Mr. Richards thinks will swallow up the influence of relativity are comprehensively required by the principles most conspicuously embodied in relativity itself.

Whether the view of criticism which I have suggested has any value or not is of secondary importance. In its favour perhaps it might be said that it conforms to the principles which require the abandonment of the older view, and it does so without losing the distinction between the enduring and the transient in literature. It recognises

* *Principles of Literary Criticism*, p. 20.

the difference between Shakespeare and Tupper, but also it provides a place for the fact that, for a short time and in one little corner of the world, Tupper established a relation with, and contributed to the life and development of, people whom Shakespeare left untouched. To deny or ignore that fact would be bad criticism and bad morals. Whether the occurrence was for good or for ill, or how far it was for each, it is no part of the business of criticism to say, but the way is open for anyone who cares to define "good" and "ill" to answer the question for himself.

This is not the place to deal with the use of hypotheses in criticism; possibly that may be given separate consideration elsewhere. This supremely powerful instrument of Science can do much valuable work in the hands of critics who know how to use it, and it has hitherto been almost completely neglected, except by those who employ it unconsciously through inability to distinguish hypothesis from fact. But this subject requires detailed consideration, and it would be better not to venture on it than to risk misunderstanding through too condensed a treatment.

CHAPTER XI

SCIENCE AND RELIGION

No discussion of human experience can evade the problem of Religion, almost insuperable though it appears. Religious experiences, estimated variously as the fools of the other experiences or else worth all the rest, have in the past entered into most distressing relations with Science, and although to-day there seems to be a general desire for reconciliation, the remarkable diversity of the grounds proposed for peace emphasise the difficulty rather than the ease with which such a consummation can be reached. It is not that there is any direct antagonism between Science and religious experience. Such antagonism is in the nature of things impossible for, as we have already seen, the religious experience is individual and Science is concerned only with experiences which are common. The difficulty is that Religion is invariably more than religious experience. It includes elements which are at any rate not obviously outside scientific discussion, and scientific discussion is always apt to lead to conclusions which religious persons are not willing to accept. Thus the trouble arises.

The natural way to look for a solution is to define Science and Religion and so locate the problem in the overlapping portion of the respective territories. Let us see what we can do along these lines.

A definition of Science is, I think, possible. I have given one in the Introduction to this book. There are many sciences, each with its own peculiarities of data and method, but there is so much in common between them that the differences are relatively unimportant. There is very little divergence of opinion about what Science is, and what divergences there are are concerned more with details than with main factors. True, there is sometimes disagreement about whether a particular study (e.g. psychical research) is scientific or not, but the difference of opinion there is not about what Science is but about what the particular study is. On the whole we can say that there is general agreement about the essential character of Science.

But with Religion it is far otherwise. If we attempt to define Religion we are baffled by the extraordinary variety of its manifestations. We can attempt to extract some essential element which is

common to all of them and without which none is worthy of the name of Religion, but even if we succeed in doing so we merely arrive at something so attenuated that no one with strong religious feelings would acknowledge it. We cannot imagine that St. Francis of Assisi would have accepted as the essence of his religion the portion which it holds in common with Mohammedanism, or that Mahomet would have regarded as inessential everything in his religion which was not to be found in Buddhism. Even if we restrict ourselves to Christianity—which historically, so far as I know, is the only religion that has entered into conflict with Science on a large scale—we are little better off. The Christianity which is held in common by the Roman Catholic, the Plymouth Brother, and the Quaker would not be accepted as sufficient by any of them. Workers in different sciences can unite in a definition of Science, but devotees of different religions cannot unite in a definition of Religion. In other words, there is no such thing as Religion; there are only religions. The failure to recognise this fact accounts for the futility of many of the attempts to deal with this problem.

The reason clearly is that Religion is largely composed of individual experiences. We might glance at a few of the elements which have been proposed as fundamental in Religion, although the fact that they must be individual is fairly evident without separate examination. One of the most recent suggestions is that Religion is the sense of the holy or "numinous" as it is called. But various objects and ideas stimulate the sense of the holy in different peoples. The contemplation of the navel is said to have excited ecstasy in certain monks, but in others it would induce either boredom or disgust. Others again, so far as we know, have no sense of the holy unless the term includes eeriness, and even then the universality of the experience is doubtful. No religion is willing to accept such an experience divorced from the object or idea with which it is associated, so that the sense of the holy becomes essentially an individual experience.

The case is not improved when we substitute sacredness for holiness. In so doing we remove the essence of Religion from feeling to valuation and so bring it nearer to the realm of reason, but it still remains outside both reason and common experience. A large variety of phenomena have been held to be sacred at different times and in different places. It is a far cry from the Sacred Cow to the Sacred Heart, and the range of sacred objects is much greater than that. No criterion is available for deciding between them except an

individual experience, and that is in fact the criterion which is actually used when we come to fundamentals.

Again, morality has sometimes been advanced as the heart of Religion, and is almost universally held to be an indispensable element of it. The "categorical imperative" has been exalted as an essential characteristic of the human mind. But an inward monitor who approves in one what he condemns in another has little claim to be regarded as identical in both. It is a simple matter of fact that the "good" of one people is the "bad" of another, and while this can be verified by anyone without leaving his own town or village, it is in the study of ethnology and comparative religions that we find it established beyond the shadow of a doubt. Morality is an individual experience in the sense which we ascribe to that phrase, and it is ineffective as the basis of a universal definition of Religion.

It is clear, therefore, that Religion—at any rate, so far as those elements of it which might claim to be universal are concerned—is made up of individual experiences. It follows that it cannot possibly come into conflict with Science, for Science operates only with common experiences. Science has nothing to say about holiness, sacredness, morality, or any other part of experience of like character. A scientist is no better and no worse as a scientist for possessing or not possessing such experiences. The possessor of them can regard with equanimity any discoveries or hypotheses which Science might make. Science and Religion are essentially independent.

Having established the truth that Science and Religion can never be antagonistic, we must now proceed to consider the well-known fact that they have on several occasions been engaged in violent antagonism. The paradox, I think, is easily resolved. The conflict has concerned a particular religion—Christianity—and not Religion in general, and it has been made possible because of an element of Christianity which no one would dream of regarding as a constituent of all religions—namely, its creed. The creeds associated with Christianity have involved, and still do involve, statements about the world of common experience which are at variance with statements made by Science. This is well illustrated by the historic dispute between Galileo and the Inquisition. This was not a dispute between Science and Religion, but between certain conclusions of Science and certain dogmas of Religion. It was not Galileo's scientific work that the Inquisition objected to, but the results of it. If he had looked through his telescope and seen something which corroborated

the Aristotelian view of the universe there would have been no trouble; he might even have been canonised: he suffered, not for his acts but for what Nature happened to be. It has been left to philosophers to criticise Science for its own intrinsic character; the advocates of Religion have criticised it only for what it has found out.

In a dispute concerning scientific discoveries or hypotheses and religious creeds, two possibilities are open. First, the representatives of Science and Religion may discuss the questions at issue, consider the relative claims to validity of the two methods of arriving at knowledge of the world of common experience, and try to reach conclusions which will be mutually satisfactory. Secondly, they may call one another names and employ whatever means of suppression are available. It is one of the most satisfactory signs of the times that the latter method—the historic one—has very largely given place to the former. This is frequently regarded as a sign that there is now no conflict between Science and Religion—a rather melancholy misinterpretation, for it indicates that there are many people who cannot conceive of a difference of belief unassociated with persecution. To prevent misunderstanding, let me say as clearly as possible that in the ensuing discussion I leave out of account entirely all consideration of the methods of conflict which have been employed; I consider only the abstract questions at issue. The question whether the Inquisition was justified in passing its judgment on Galileo is as irrelevant as the relative merits of Lifebuoy and Sunlight soaps. Our business is with ideas, not with events.

Let us, then, consider how a religious creed can enter into conflict with Science. A creed is literally a belief, and it is open, I suppose, to anyone to believe what he will or what he can; it is his own private affair. Before he can enter into conflict with Science, two further conditions must be satisfied. First, his beliefs must concern actual or potential common experiences; secondly, he must hold them to be true in the sense in which the scientist holds the conclusions of Science to be true.

It may be doubted whether a religion without a creed is possible, but it cannot be doubted that a creed which does not satisfy these two conditions is possible. Any creed which is simply a unification of one's own experiences is an example. Thus, if a man believes that his life is controlled or guided by a divine Power, he holds a creed which makes no assertion about common experiences and therefore cannot be accepted by Science. If, however, he regards this Power

as a potentially common experience, and asserts that other people's lives are or may be guided thereby, he is raising a question on which Science has a right to be heard. If the scientific evidence is against the existence of such a Power, the conflict begins.

We shall presently consider some specific problems of this kind. We cannot, of course, be exhaustive, for there are an endless number of them, but there are a few of particular moment which not only will serve as examples, but also demand consideration on their own intrinsic merits. And here let me say that I make no attempt to solve these problems, or even to state them in their most vital form. All that I am concerned with is that part of them which touches Science. I shall try to estimate how far Science, by virtue of its own essential nature, can affect their solution. What is left of them after that can be handed over to Philosophy to deal with. I do not deny its importance, but it is not what I wish to discuss.

Before coming to the individual problems, however, there is one point of general application which it will be useful to consider. What kinds of pronouncement can Science properly make on questions involving actual or potential common experiences? There are various possibilities. On some questions its answer can be final. Thus, if the problem is whether or not a heavy and a light body will fall to the ground from a given height in equal times, the experiment can be made and there is no appeal against its decision. It can be readily seen that this applies to any question which the disputants are agreed is independent of the particular occasion of the experiment. It may be, however, that for practical reasons the answer of Science cannot immediately be given. The problem of life after death, in its usual form, may be cited as an example, in order to fix our ideas. In such cases we must either suspend judgment or else face the very difficult question: How far can knowledge, of the same character as scientific knowledge, of the world of potential common experience, be obtained by "intuition" or "revelation" or whatever other equivalent word is preferred? I do not propose to discuss this question because it is outside my sphere; it is a problem of epistemology, and that is not Science. All that we need notice is that, whatever answer be given to it, Science can make no assertion on the point at issue until such time as it can deal with the matter in its own way, *i.e.*, make the supposedly potential experience an actual one.

Problems which are not independent of particular occasions are on a different footing. Science can make definitive statements about

the rotation of the Earth on its axis, but if it is asked whether, a few thousand years ago, the Sun stood still in the midst of heaven for about a day, it can answer only in terms of probability. The probability may be overwhelmingly in the negative, but still the answer is of a radically different character from the answer to the problem about falling bodies. By "probability" here I mean not so much the calculable mathematical probability as the tendency of mind engendered by our general scientific experience of the universe, and this, historically speaking, is very close to the heart of the controversy between Science and Religion. The whole of the great dispute about evolution in the last century turned on the different weights attached to the evidence by thinkers reared respectively in a scientific atmosphere and an atmosphere of bibliolatry. It is not so absurdly meticulous as it seems to distinguish between probability and certainty in regard to Joshua's miracle.*

In considering the relation of Science to religious creeds, then, we must recognise three attitudes which Science can take, according to the character of the creed in question: it can make a final pronouncement, it can reserve judgment, or it can influence the probability with which a particular dogma is invested. We may now turn to the dogmas and see which attitude is the appropriate one in each case.

Let us begin with the problem of miracles. This was a very hotly debated subject in the nineteenth century, but it is now clear that the root of the controversy was not the credibility of miracles at all, but mainly the status of the Bible as a record of matters of fact. Huxley and Gladstone, quarrelling over the Gadarene swine, did not really care about the actuality of the particular incident in which those unfortunate animals were alleged to have participated; what they were actually concerned with was the interpretation of holy scripture as an impregnable rock or an imperfect human document. Miracles such as these were not necessary to Christianity. If they had been omitted by the evangelists there would have been no difference in the religion of anyone, and we know now that Christians who realise that their faith is not indissolubly bound up with the infallibility of the Bible are not inclined to attach any importance to them. But since they had not been omitted it was felt that they had to be upheld, otherwise there would be no guarantee that the essential parts of Christianity were surely grounded in historical fact. The problem

* Compare the remarks on hypotheses relating to past events, p. 46.

of such miracles as these, therefore, is subsumed in that of the authority of holy scripture.

I said that the root of the controversy was *mainly* the status of the Bible. That did not exhaust the significance of the matter, however, for there was also involved in it the quite irrational idea that the ability to perform a miracle was a guarantee of authority in matters of conduct and belief, and that somehow the credentials of Jesus were the authenticity of his miracles. This idea is not active to-day, and we will not dwell on it except to note that although it has left the religious sphere, its spirit still walks abroad in public life. When we find our newspapers inviting men who have knocked a golf-ball into a series of holes in the smallest number of attempts to express their views on the problem of survival after death, and when prominent geometricians or novelists are expected to have something of value to say about the philosophy of democracy, we have no difficulty in recognising the type of mind which regarded the miraculous draft of fishes as an argument for the Sermon on the Mount. But we are not concerned with that matter now.

The infallibility of the Bible likewise is not such a vital question now as it has been in the past: nevertheless it is by no means settled, and since it has had great historical importance (it was the central feature of the epoch-marking disputes of the seventeenth and nineteenth centuries, which are the main landmarks in the history of the conflict between Science and Religion) we cannot dismiss it altogether, althought our remarks on it will be brief.

This is clearly not a subject on which Science can make any statement in general terms. Biblical infallibility implies the inspiration of the writers, and inspiration in this sense is not a term which has any meaning in Science. Science can deal only with common experiences, and inspiration is not one of them. There is, however, a scientific meaning in the question whether the Bible is accurate as a historical record, and this can be dealt with scientifically only by considering each statement in it in terms of all the available evidence, no evidence being admissible which is not entirely grounded on common experience. We are thus thrown back again on the question of miracles, not as an independent question, however, but as the most debatable aspect of the general question of biblical accuracy. And here the position of Huxley still remains unassailable: no miracle can on scientific grounds be said to be impossible, but the observed regularity of Nature may make its occurrence so im-

probable as to require very strong evidence in its favour before it can carry conviction. To the scientist *qua* scientist it is a matter of weighing evidence of different kinds: on one hand, the expectation aroused from the observation of Nature now; on the other, documentary evidence. These influences on judgment are incommensurable, and the decision must therefore be a matter of individual predilection, which is not scientific. The scientist, therefore, must officially remain agnostic on the question, whatever he may think as an individual man.

But there are certain miracles which merit consideration in their own rights, so to speak, and not merely as part of the general question of biblical infallibility. Christians who no longer look upon the Bible as anything other than a collection of humanly written books of varying degrees of accuracy and value, would still insist on the actuality of the virgin birth and resurrection of Jesus. The Apostles' Creed, which is repeated in our churches every Sunday—and, we must assume, believed by some of the worshippers at least—is a confession of faith in several miracles. These miracles concern matters on which Science has something to say. We must therefore consider the limits of scientific pronouncement in such matters. This raises the whole question of what we mean by a "miracle."

A miracle is usually thought of as an interruption of the order of Nature. In the ordinary course of events a particular combination of circumstances is invariably followed by a certain result (e.g. a cricket ball hit into the air invariably returns towards the ground), and we express the sequence of events by what we call a "natural law." If, however, on one occasion the expected result did not follow, although the circumstances appeared to be unaltered, that would be regarded as a miracle or a violation of the natural law. Such occurrences are asserted in the Apostles' Creed, and have been declared by some to be impossible. What is the scientific attitude towards them?

In the first place, we may notice that they are very frequent. The face of the heavens remains unchanged for decades, and suddenly a new star blazes forth and dies down again; a volcano remains dormant for centuries, and suddenly bursts out in violent eruption; a swarm of fruit flies produces offspring true to type for many generations, and suddenly a "mutant" appears; the Sun shines in its accustomed course for centuries, and one day for a short space it becomes eclipsed, and then resumes its ancient splendour; and so on. All these —and thousands more could be enumerated—are interruptions of

the observed order of Nature, yet they are well known to Science and no one in his senses will suggest that they are impossible. It is by seeing how Science deals with them that we can best see its attitude to the possibility of other miracles.

Now Science has a very simple way of treating miracles; it simply revises its statement of the order of Nature so that the unexpected occurrence is included as a possibility. For instance, it no longer says that the Sun's rays fall uninterruptedly on the Earth, but merely that the Sun emits its rays uninterruptedly in all directions. The eclipse is then accounted for by the interposition of the Moon between the Earth and the Sun, and the new law of Nature is not violated by the extraordinary occurrence. By this device, Science makes a violation of the order of Nature not merely impossible but inconceivable: you cannot violate a law of Nature by an unexpected occurrence because the occurrence is absorbed into the statement of the law.

The idea, formerly so prevalent, that Science can reveal conceivable events as impossible was based on a false notion of a scientific law. A law of Nature is not some ruling principle inherent in the texture of things; it is simply an expression, in the most suitable terms, of what we observe. When we observe something inconsistent with such an expression we abandon the law in favour of a more comprehensive one. Laws of Nature are constantly being martyred in this way, and their blood is the seed of Science. It is a contradiction in terms to speak of an event as a violation of a law of Nature. Nature is the sum-total of our common experiences and an event is one of those experiences.

There is, of course, a great difference between the recognition of the possibility of miracles and belief in the actuality of particular ones, and we are probably as far as ever from obtaining scientific evidence for the miracles of the Apostles' Creed. Yet perhaps they do not seem so repugnant to the scientific temper as once they did. Consider, for example, the one which is the most objectionable from this point of view—the resurrection of the body. It is the creed of one eminent scientist at least that there is an entity which we may call the "soul" which uses matter as its instrument and which survives death. Whether this creed is scientifically permissible or not, there is no doubt that it may conceivably become so. If, then, the soul survives death (as many who "do not believe in miracles" would assert), there is no obvious reason why it should not retain

the power of shaping a body for itself, and if the soul is the same we might expect the new body to resemble the earthly one. It is beside the point to say that it is not a similar, but the identical body whose resurrection is claimed. What characterises the earthly body is its *form*, not its actual protons and electrons; my body to-day may contain not a single one of the protons and electrons which composed it twenty years ago, but acquaintances of that time still recognise me and I do not think of myself as having acquired a new body.

These considerations, it should be needless to say, throw no light at all on the actuality of bodily resurrection, but they help to illustrate the fact that there is no essential distinction between miracles, as they are usually understood, and the kind of unexpected fact that is constantly cropping up in scientific investigation. And they do more. We have seen that Science does not declare against the possibility of any particular miracle, but only influences the probability which we attach to its having happened. We see now that that influence is not a constant one, but may vary from time to time. Miracles which seem in the highest degree unlikely at one period of scientific development may appear quite probable at another. The miracles of healing afford an obvious example, and even within the realm of Science itself, "rational" and "superstitious" are labels which not infrequently change places, as the words "telegony" and "telepathy" may remind us. We may repeat that the distinction between probability and certainty in these matters, however pedantic it may appear in a particular epoch, can never be safely ignored.

But if Science thus refuses to place its ban on the possibility of miracles, it by no means thereby concedes to the orthodox theologian all that he requires. Science, by giving up all claim to the discovery of directing principles in Nature, is stooping merely to conquer; it enlarges the possibilities of Nature which is its own field of operation. But what the theologian requires is rather the recognition that miracles are *impossible* by Nature and therefore demand the acknowledgment of the supernatural. He is interested not so much in the actual occurrence of the miracle as in its *significance* as a revelation of an omnipotent Being.

This brings us to another great problem—that of the existence of God—but before dealing with it generally we must notice that this view of miracles as revelations of God depends on the *a priori* assumption of an "order of Nature," which we have already seen (p. 20) is not a part of Science. Such an assumption—which is that of a

quality imposed on Nature from without—is already equivalent to an assumption of God, and of a God with precisely the same qualities as those which would be established by miracles. There is no need to look to a violation of the order of Nature for a revelation of God when an equal argument for his reality is available in the existence of that order..

The general problem of the existence of God is perhaps the most vital of all religious problems. The difficulty, as usual, turns largely on the meaning of terms, and we may begin by noticing that the word "God" is used sometimes to represent an experience and sometimes to represent an idea. God may be a very present help in time of trouble or an absolute First Cause. Let us consider in turn the two cases in their relation to Science.

The God who is an experience exists in the scientific sense only if the experience is a common one. The only meaning "existence" has in Science is "common experience." The Eiffel Tower is said to exist because directions (in this case, time and place) can be given, by following which any normal person can experience the Eiffel Tower. We have already dealt with this point in Chapter IX, and the considerations advanced there lead us to the conclusion that God does not exist in the sense in which the Eiffel Tower exists. He is an individual experience and not a common one.

It is perhaps advisable to repeat that this does not imply that He is an illusion. Those who claim that must first define what they mean by "illusion"; it is not a word which has any meaning for Science. When we say that from the scientific point of view God does not exist, we mean nothing more and nothing less than that He is not a common experience. Toothache does not exist scientifically for the same reason, and there is nothing whatever in the scientific pronouncement to make the experience of God any less vivid or real than toothache. There are, I believe, people who say that toothache is not real. Presumably they know what they mean, though I do not, but I think that most people who have suffered from that complaint will accept the illustration as sufficiently distinguishing the scientific conclusion from what is usually understood by atheism. If anyone wishes to challenge the statement that God does not exist for Science, he must give instructions which anyone may follow and automatically receive an experience of God. Instructions professing to have this quality have been and still are being given, doubtless with some success, but certainly not with universal success.

Thomas Hardy's *Impercipient* still speaks for a multitude of genuinely distressed persons:

> He who breathes All's Well to these,
> Breathes no All's Well to me.

So long as this is true the God of experience does not exist for Science.

We may here emphasise, however, a point which has been hinted at before; namely, that so long as Religion makes no assertions regarding the world of common experience, it may employ scientific methods with perfect justification. The individual man may correlate his own experiences, whether of common or individual nature, in a single whole with complete validity, and if in so doing he finds the assumption of God necessary, his position in adopting it is impregnable. He may not therefore assert the existence of God as a potentially common experience, but to him God may be not only as necessary but as legitimate a Being as the objects of his sensuous perception. The man whose position is indefensible is he who asserts the existence of God on the basis of the experience of others. To him God is neither an individual experience nor a common experience, and he has neither religious nor scientific sanction for his assertion.

The God of common experience, then, does not exist, but there is the God of reason to be considered. Science is concerned with rational correlation as well as augmentation of experiences, and in the process, as we have seen, it employs hypotheses which, if verifiable, may become common experiences. Perhaps the existence of God may be established in that way.

It is, I suppose, possible that the correlation of common experiences may, in course of time, require the hypothesis of a God; at present it has no need of such a hypothesis, and so long as physics, at least, retains its present character there is no sign that it ever will do so. If, however, it does, we may feel fairly confident that the hypothetical God will not be such as to satisfy the desires of the theologian. The idea of a personal God is irrelevant in physics, not because it is unsuccessful but because it is too successful, not because it cannot correlate our common experiences—it correlates them all at one fell swoop—but because it cannot correlate them rationally. If we assume a Being of whose unlimited will Nature is simply the expression, then clearly there is nothing more to be said. If events happen regularly it is because he wills it so, but at any moment he may change his mind and then they will happen irregu-

larly. Whatever happens, however, everything is unified in him, just as our own arbitrary acts are all correlated in us. Science is thus at the same time completed and destroyed. This is, in fact, what would occur if the second kind of indeterminacy referred to in Chapter VIII were established throughout Nature, but the experience we have already obtained makes that impossible.

If Science introduces the hypothesis of God, therefore, it will represent him not as a personality whose will is revealed by the events of Nature, but as a strictly defined concept with no properties other than those necessary to correlate the common experiences in virtue of which he is postulated. How much may be included in this it is impossible yet to foresee. So long as physics is divorced from biology and psychology the hypothesis cannot contain very much, but if the various sciences should become united and in the wider generalisation such concepts as that of "purpose" should embrace the concept of "number" in terms of which physical laws are now expressed, it may be found necessary to postulate a hypothetical God who will approximate more closely to the requirements of theology than seems possible now. It is conceivable that this God may become the object of a common experience through the discovery of certain processes which will automatically reveal him to the inquirer in a kind of communion, but if so the experience will have to be something much more definite than the religious experiences of individuals to-day, for they are characterised most obviously by their variety. All this is, of course, highly speculative, but it is as important to consider what Science may do in the future as to discuss its present state.

Much interest has lately been aroused in the view expressed by Sir James Jeans* that, "the Great Architect of the Universe now begins to appear as a pure mathematician." Sir James is himself an eminent mathematician, and his statements are widely accepted as the embodiment of current scientific thought. In some respects, of course, they are—as, for instance, in his fascinating exposition of the theory of relativity—but it cannot be too clearly understood that when he makes the remark just quoted, and others relating thereto, he is speaking for himself alone, and is expressing not a scientific deduction but a metaphysical one, requiring for its validity an assumption for which Science gives no warrant. It would not be difficult to show, I think, that in some respects his metaphysical conclusions are actually at variance with his scientific beliefs. For

* *The Mysterious Universe*, p. 134.

example, he deduces the character of the "Great Architect of the Universe" from the present aspect of theoretical physics; he holds that theoretical physics "now makes it appear that nature abhors accuracy and precision above all things";* nevertheless he concludes that Nature "does not model her behaviour, so to speak, on that forced on us by our whims and passions . . . but on that of our thinking minds."† and the character of the "Great Architect" is prescribed accordingly. It is not of metaphysics, however, that I wish to speak here, but of Science, and the important fact for us to notice is that the "Great Architect of the Universe" is not a scientific concept. He is brought into the argument without introduction, as if his existence were a matter of previous consent, and what Science (or, rather, physics) is required to do is to say what he is like.

I should be the last to deny the right of Sir James Jeans or anyone else to hold that the universe was or is designed by what, with our limited comprehension, we can only describe as a "Mind," but I think much misunderstanding would have been avoided if he had emphasised the fact that this belief is independent of Science. One has only to condense the apparently scientific argument to see how inconsequent it becomes: The universe appears to be composed of thoughts in our minds; therefore we are thoughts in its mind: in theoretical physics we do nothing but think mathematically; therefore the Architect of the Universe can do nothing but think mathematically. The act of looking to physical thought for information about God can be justified only by a metaphysical pre-establishment of the existence of God, and the assertion that all that we can say about God is contained in physical thought would appear to be unjustifiable by any consideration at all. If God is recognised on non-scientific grounds, his character should be at least partly read in the medium through which he is recognised. If morality requires him then he will have moral attributes; if art requires him he will have æsthetic attributes; and if science requires him (and at present it does not) he will have scientific attributes.

God, freedom, and immortality, the "metaphysical trinity," occupy a prominent place in religious discussion, and it is appropriate that we should consider the relation of Science to each of them. Freedom of the will, it may be noted, presents a different kind of problem from the existence of God, for although Christianity, at least, has stood consistently and unequivocally for the existence of God, its

* *The Mysterious Universe*, p. 26. † *Ibid.*, p. 135.

position with regard to freedom has been much more ambiguous. While freedom would seem to be necessary for moral responsibility, the doctrine of predestination seems inconsistent therewith. Science, therefore, would appear to be not so much a potential opponent of Christianity in this matter as a possible arbiter.

It is clear, however, from the point of view adopted here, that Science has, and can have, nothing at all to say on the question. To Science, will is a quality of mind, and mind is an abstraction from behaviour (cf. pp. 24–25). It is a sort of parameter in terms of which observations can be expressed, and its only characteristics are those which enable it to express observations. Freedom is not such a characteristic; it is a metaphysical notion. In Science we can no more ask whether the will is free than whether time is free; both are abstractions of precisely the same character. Whatever meaning there is in the problem—and the history of the controversy suggests that it is often merely a matter of words—there is no scientific meaning.

Immortality, however, stands on an entirely different footing. That is a question on which Science may have something very vital to say, although whether it will ever become clearly articulate or not is of course quite another matter. From our point of view the question that confronts us is this: Can common experiences, of a kind similar to those which we used to regard as characteristic of Mr. A, come to us after the death of Mr. A? Such experiences do not normally come, but that may be because we have not established the right conditions. Psychical research, from this point of view, may therefore be a perfectly legitimate scientific procedure, although it does not follow that it is always conducted on true scientific lines. Certainly, since its findings are estimated so variously by scientific men, it cannot be said that Science has at present any definite pronouncement to make on the subject. In any case, it should be noticed that while this kind of research may conceivably establish the reality of survival, it can never give a definitive answer in the negative.

The main difficulty of the problem, however, is ultimately that biology (or psychology) and physics are at present independent sciences, employing abstractions and hypotheses so far unrelated to one another, and when we inquire into the question of survival we are mixing up these so far unrelated ideas. Mind is an abstraction of psychology and time is an abstraction of physics, and when we ask if the mind persists through time we are asking a question which may have no meaning. During life, Mr. A has, in the present state

of Science, to be regarded as a dual phenomenon—a material body on one hand and whatever distinguishes him from the dead Mr. A on the other. Time and space are characteristic of the former, and the living Mr. A exists in time and space because he is a material object. After his death he remains a material object, occupying space and extending through time, but the behaviour from which his mind was abstracted has ceased. We think it meaningless to ask if his mind still occupies space, but somehow we think it might occupy time.

The theory of relativity has taught us a great deal about time. We know that the term "now" is ambiguous except for bodies in contact with one another; the present instant to us may mean any time within nearly two million years to an inhabitant of the Andromeda nebula. Again, a being moving with the speed of light may experience instantaneously what to us occupies a lifetime. Are we sure, then, that we know what we mean when we ask if Mr. A is alive now? We cannot go to the Andromeda nebula or move with the speed of light, but we can die, and that is possibly at least as revolutionary an adventure. It may be that the act of dying, which occupies an instant to the onlooker, stretches for the dying man through eternity, somewhat in the manner adumbrated by Tolstoi in *Sevastopol.* We have no right to assume that our ingenuous association of the independent abstractions, mind and time, has any meaning in Nature. To do so is perhaps to repeat the error of the nineteenth-century physicists, who saddled hypotheses with the characteristics of phenomena, by saddling psychological abstractions with the abstractions of physics.

Science, then, can at present make no authoritative contribution to the problem of survival. It may eventually produce positive evidence on the matter by means of psychical research, or, alternatively, restate the problem in such a form that there will no longer be any possibility of conflict with the requirements of Religion. And in view of the second possibility, it may not be irrelevant to bear in mind that Christian theology has always seemed to recognise instinctively that spiritual matters are somehow removed outside time; eternity is by no means synonymous with infinite extension in time. At any rate, it is clear that whatever dogma may be held on this matter, Science can at present have nothing final to say in its favour or disfavour. The problem of survival offers, perhaps, the least ground for conflict of any of the great problems with which Science and Religion are jointly concerned.

The reader may remark that all this discussion brings us no nearer to a solution of the various questions raised. That is probably true, but it does help, I think, to locate the problems in the divided territory of human thought. It shows that a large part of the so-called antagonism between Science and Religion is really independent of Science; the controversy lies between Religion and Philosophy, which is quite another matter. With regard to the residue, which does concern Science, the trouble is not so much a direct contradiction as the creation of opposing tendencies of mind; that which seems probable to one breathing a religious atmosphere will sometimes seem improbable to one nourished on Science. It is here that the ultimate conflict between Religion and Science arises, and when once this is realised discussion becomes useless, for the origin of the divergence lies beneath discussion. That is why, in spite of the volumes that have been spoken and written on the subject, very little real advance has been made.

I doubt if any scientific man whose religion is not a permanent guiding principle in his life, or who is not in frequent and intimate contact with people who are so guided, can hope to ease the strain between the scientific and religious mentalities. He may show with crystal clearness that certain religious dogmas are completely independent of the essence of Religion, or that experience is one thing and the intellectual expression of it quite another, and he may be perfectly right and be doing work of great value to future generations. But for the present the lives of individual Christians confute him point-blank. It is a simple fact that the mature Christian with established convictions cannot separate his experience from his creed. He not only imagines, he *knows*, that if he abandons his theology his experience will go with it, and he also is perfectly right. We may speak of the deepening of religious experience through progress in theology, and quote Tennyson:

> He fought his doubts and gather'd strength,
> He would not make his judgment blind,
> He faced the spectres of the mind
> And laid them: thus he came at length
> To find a stronger faith his own.

But the man of whom that is written had doubts to begin with. The man who has no doubts cannot give up his theology without religious shipwreck. We may call him a blind bigot, or credit him with a

higher wisdom than that of the mere intellectualist—that is a matter of opinion; but what is undeniable is that he is a fact.

For this reason it seems to me that the problem becomes at bottom a psychological one. It is not in the objective character of Science and Religion that it will be resolved but, if at all, in the nature of the mind itself. If such a resolution is reached it may appear that the particular problems we have discussed are of little account, but that possibility belongs to the future. In the meantime we can only discuss them from our own point of view and hope that the discussion will not be entirely idle.

INDEX